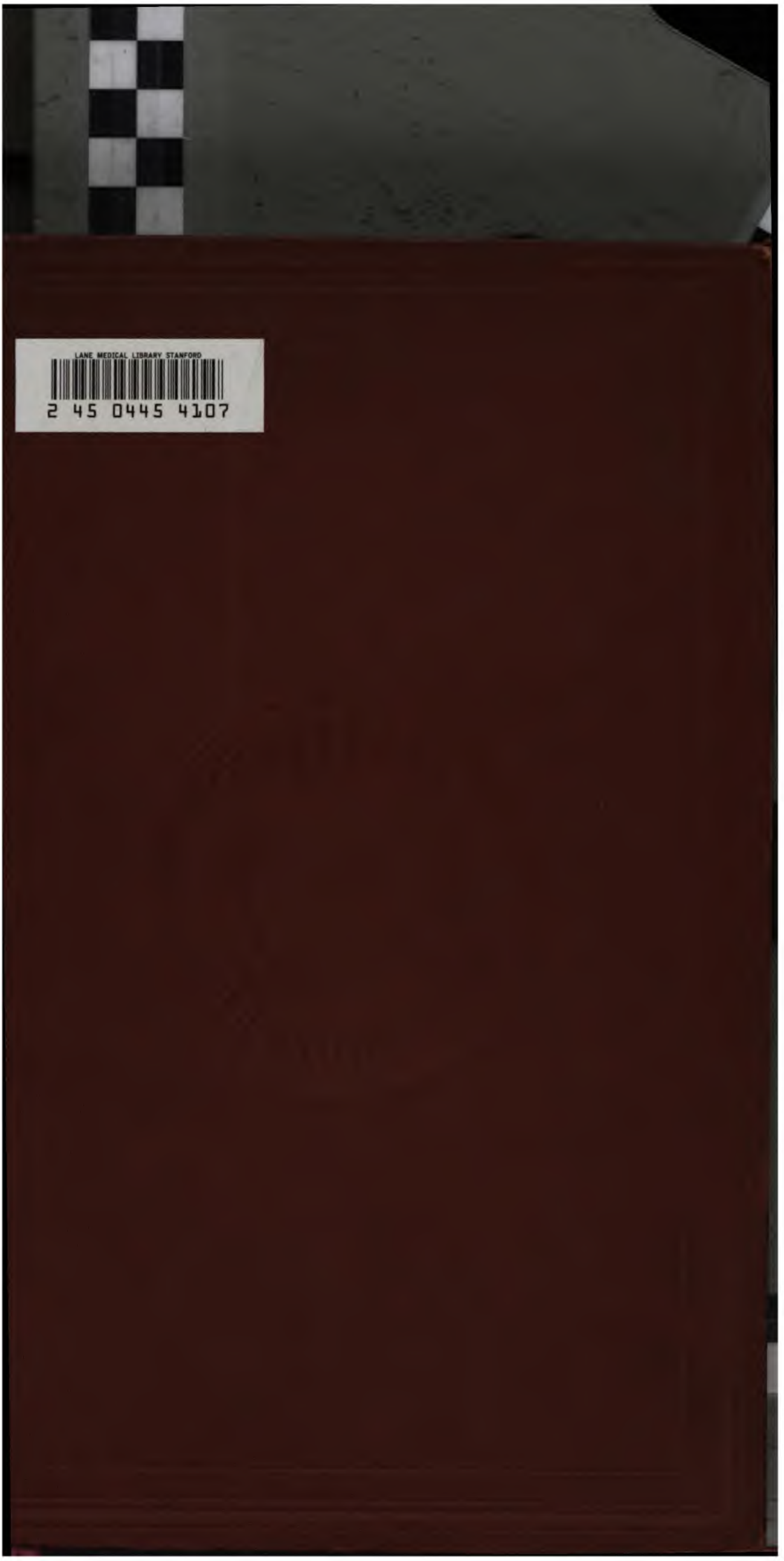

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ENTOZOA.

ENTOZOA

BEING A

SUPPLEMENT

TO THE

INTRODUCTION TO THE STUDY OF

HELMINTHOLOGY

BY

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PREFATORY NOTE.

NEARLY five years having elapsed since the first appearance of my Treatise, the Publishers have thought that the issue of a Supplement—embracing a special and general Index, together with additions to the Bibliography and a record of researches conducted since the year 1864—would not only prove acceptable to those who have already possessed themselves of the work in its original form, but would also be likely to enhance its value, independently; and for myself, there is the advantage which it thus affords me of offering my sincere acknowledgments to the score or more of "reviewers" who have been pleased to honour my previous efforts with their favourable comments. The reviews in question have necessarily proceeded from unknown pens; but, as many of them contain useful information, I have supplied references to the more important under the title "Anonymous" in the Supplemental Bibliography.

The progress of Helminthological science abroad has not been so striking of late years as formerly, owing partly to the circumstance that several well-known investigators have turned their attention to other branches of zoology. My thanks, however, are due to several distinguished men who have furnished me with valuable communications. In this regard I am under much obligation to Dr. Rudolf Leuckart (formerly Professor of Zoology and Comparative Anatomy in the University of Giessen), and to Professors Claparède (Geneva) and Cornalia (Milan); also, in a scarcely less degree, to the late Dr. Diesing (Vienna), to Dr. Krabbe (Copenhagen), to Dr. Davaine (Paris), and to Dr. Peter Olsson (Lund).

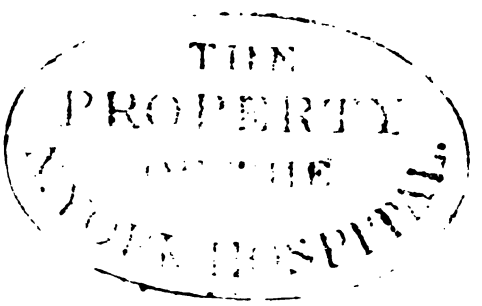
To authors of papers written in the English language my indebtedness is

too large to be acknowledged in detail; nevertheless I may, in this respect, particularise the names of Professors Aitken, Bastian, and E. Nettleship; with those of Drs. Murchison, Baird, Waring, P. H. Macgillivray, Macalister, Messrs. Alexander Agassiz (U. S.) and T. S. Ralph (Australia). For valuable MS. notes and suggestions I am likewise indebted to Prof. S. Percy, M.D., New York; to Prof. Gairdner, of Glasgow; and to Assist.-Surgeon Henry C. Brodrich, 1st Corps Central India Horse, stationed at Augur, in Malwa.

As regards specimens, I am under particular obligation to Mr. Charles W. Devis; and in this connection I have also to thank Messrs. Hennah (Brighton), Slade (Buckingham), Tate (Wakefield), Higginson (Liverpool), Tegetmeier (London); also Dr. John Barker (Dublin), Drs. Norton (Wateringbury), Leared, Wilson Fox, and Duffin (for Spurious Entozoa), Dr. S. Holdsworth (Wakefield), Drs. Murie, Weber, and John Millar (London), Dr. Ormerod (Brighton), and, in an especial manner, Robert Swinhoe, Esq., H. B. M. Consul at Amoy, China.

T. S. C.

84, Wimpole Street, London,
May, 1869.



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ENTOZOA.

I.

ON THE HISTORY OF THE DISCOVERY OF TRICHINA SPIRALIS.

In the twelfth chapter of the second division of this work, I have glanced at the history of the so-called flesh-worm. Since the time of publication, however, circumstances have necessitated, on my part, a thorough and exhaustive examination of the very copious literature of this subject, both home and foreign. I have, certainly, no reason to regret the trouble taken, as it has enabled me to express, in reference to questions of priority, convictions which I have all along entertained, but which, from the peculiar surroundings of the case, I had hitherto been debarred from stating in their fulness. In a word, it may be said that the discovery of the Trichina, as a human parasite, rests essentially with Mr. Paget, F.R.S. This truth is proven by the discoverer's own letter, as well as by the joint communication of "Two former Presidents of the Abernethian Society, connected with St. Bartholomew's Hospital." The documents, together supplying all the missing links in the chain of evidence, were published by their respective authors in the "Lancet" for March 3rd, 1866. In effect, they authoritatively confirm the views I had expressed in a "foot-note" to a paper previously published in the same Journal. The true and essential facts of the case

are none other than those here repeated; the present record being, in part, the same as the one above referred to. It is only right to add, in passing, that the question of priority, now fully and fairly established in Mr. Paget's favour, in no way detracts from the high merits of those other distinguished physiologists who have since made scarcely less important discoveries in connection with this truly remarkable parasite.

To the partially informed it may appear a work of supererogation to offer renewed details on this subject; but the wide-spread interest created by formidable outbreaks of the flesh-worm disease, together with the evident confusion still existing in the above expressed relation, render it most desirable that all students of helminthology should possess clear and definite ideas regarding the true sources whence our knowledge of the parasite has been derived. When, not long ago, a writer in an ably-conducted newspaper put forth a statement to the effect that the history of Trichina and Trichiniasis was "not yet clearly made out," and further, that "extensive series of experiments had led to no definite result," I felt called upon to correct this misapprehension, by addressing a letter in reply. As I referred the general reader to the best authors, I hoped, at the time, the matter would drop; but another writer supplemented my statement by pointing to Professor Owen as the discoverer of Trichina. This brought on further discussion, the particulars of which it is quite unnecessary to recapitulate. In the course of correspondence I expressed the opinion that the "little bodies" first noticed by Tiedemann in the year 1822,—thus anticipating Mr. Peacock (whose name had been incidentally mentioned) by six years,—were neither more nor less than those calcified and degenerated little cysts which we now know to be the so-called Trichina capsules.* "Happily," I remarked, "the discovery of the actual 'flesh-worm,' itself, will continue to be imperishably associated with the names of Owen, Paget, and Wormald, whilst the

* See Froriops' "Notizen," the title-page of which bears the date of 1822; but, in point of fact, the date of publication of the notice in question was August, 1821.

scarcely less important discovery of the lemon-shaped or calcified capsules must permit a similar bracketing of the names of Hilton, Peacock, and Hodgkin." The *merit of priority of discovery is gradational both in degree and in kind*, but in order to establish the relative degrees of priority involved, it is necessary to display entire accuracy of criticism. Respecting the main facts, I believe the following to be decisive:—

So far as regards the parasite itself, it must be apparent to all who have perused the standard treatises and monographs of Küchenmeister, Leuckart, Davaine, and Pagenstecher, that there has been no indisposition on the part of continental writers to render to our countrymen their legitimate meed of praise; whilst, at home, naturalised foreigners, such as Dr. Thudichum and Althaus, have honourably followed the same track. All these authors' works I have before me; their several historical records being substantially the same. In this treatise on "Entozoa" I purposely avoided giving a lengthened account of the history of Trichina, because I supposed "the circumstances attending the discovery of this parasite" were "so familiar to the English reader" as to render such a step unnecessary (p. 335).

At the time to which I have previously alluded, it was even hinted that I had been attempting to take from our countrymen the merit of the discovery of the trichinous disease. It will be seen that such an intention was far from my thoughts; for, at p. 335 of the present work, I have said:—"To my mind this discovery is one of the happiest to which Englishmen can point."

The passage in Frorieps' "Notizen" which seems both to Henle and myself to describe, somewhat rudely perhaps, a genuine case of Trichina, stands as follows:—

"Tiedemann fand bei der Leichenoffnung eines Mannes, der ein starker Branntwein-trinker gewesen, und nach mehreren heftigen Gichtenfällen an Brustwassersucht gestorben war, in den meisten Muskeln, vorzugleich an der Extremitaten weisse steinige

Konkremente. Sie lagen zwischen den Faserbündeln in Zellgewebe; haufig auch an der Wänden der Arterien waren zwei bis vier Linien lang und rundlich. Die chemische Untersuchung welche Gmelin veranstaltete zeigte darin 73 Theile phosphosauren Kalk, 7 Theile kohlenstoffsauren Kalk, und 20 Theile einer thierischen, dem Eiweiss oder Fibrin ähnlichen Materie."

This paragraph I translated thus:—"At the post-mortem examination of a man who had been a great brandy-drinker, and who died from thoracic dropsy after several severe attacks of gout, Tiedemann found white stony concretions in most of the muscles, especially at the extremities. They lay in the cellular tissue between the fibre-bundles; frequently also attached to (or near) the walls of the arteries, being from two to four lines long, and roundish. The chemical examination conducted by Gmelin yielded seventy-three parts phosphate of lime, seven parts carbonate of lime, and twenty parts animal matter, resembling albumen or fibrin."

Now, in regard to this passage, it has been correctly stated that Leuckart denies that we have here to deal with Trichina. In this particular opinion Leuckart stands almost alone—I mean amongst recognised helminthologists who have honestly investigated the matter and have conducted experiments with Trichina. I am sure neither Pagenstecher nor myself would willingly detract one iota from Leuckart's authority; but I hold that we are both entitled (as original and, for many years past, laborious workers in the field of helminthology) to express a contrary opinion, in accordance with our convictions. Pagenstecher, referring to the views entertained by this distinguished investigator, remarks:—"Leuckart hat irrthümlich dieses Citat fur falsch erklärt."* Henle appears to have been the first to have pointed to the passage of Tiedemann as describing an example of Trichina; and in this

* See foot-note to p. 4 in his recent work "Die Trichinen." Leipsig, 1865. This beautiful memoir contains a report of experiments performed at the Zoological Institute of Heidelberg, by Professors C. J. Fuchs and H. A. Pagenstecher, by order of the Baden Grand-ducal Board of Trade.

reference he has been followed by Diesing, Küchenmeister, Davaine, Thudichum, and Aitken. But I am under the strongest impression that only Henle, Leuckart, and Pagenstecher examined the original passage for themselves before they placed their references on record.

It would hardly be fair to say that Pagenstecher dissents from Leuckart's view of the question of priority. To put the matter clearly, however, it is necessary to state precisely what he really does say. Speaking of Tiedemann's notice, he writes ("Die Trichinen," s. 4):—

"This communication was also refered by Henle to such a development when he subsequently found Trichina; and in this sense it was afterwards received by Diesing, Küchenmeister, and Davaine. But it has been rejected by Leuckart on account of its size (from two to four lines) and seat of the concretions. True, it has never yet been observed that the capsuled Trichina (not measuring a tenth part of that diameter) subsequently constituted centres of gouty deposit exceeding their own bulk; nor is it likely that they should. Seeing, however, as we often do, that errors repecting size have crept into works on Trichina, we shall not need to lay much stress upon these statements; still less so since the notice is very superficial, and its character is essentially of a physiologico-chemical nature. But this, at least, seems to us decisive, that when Bischoff, at Heidelberg, wrote on a case which occurred in Heidelberg, not one single word was mentioned respecting a former case, if such should have happened, although Tiedemann and himself were on terms of close intimacy. Our hope to be able to produce a further decision by the examination of the preparation preserved in our anatomical collection has been disappointed; nothing of the kind could be found."

From this extract it seems that my view does not gain Pagenstecher's entire support; although, in a foot-note, he remarks that Leuckart has erroneously declared Henle's quotation to be false. Leuckart, perhaps, only referred to the source of the notice.

Pagenstecher further goes on to discuss Klencke's claims to the discovery of Trichinæ. He says :—

"Klencke has asserted that he had already drawn Trichinæ in the year 1829, and that he had seen them again in 1831. This subsequent statement has no kind of confirmation, The unreliableness, mistakes, and self-deceptions in the helminthological writings of Klencke have been repeatedly exposed some twenty years ago."

As I have said, I cannot but believe that Tiedemann's notice describes Trichina capsules. The real difficulty is the size there given. With me, this goes for next to nothing; for, as Pagenstecher himself has pointed out, similar errors have been made by able writers. Thus Virchow erroneously described intestinal Trichinæ to be four lines in length; whilst, on the other hand, Vogel spoke of free Trichinæ as one-hundredth of an inch long—"the former statement being just as much too exaggerated as the latter is too small." Whatever be the true state of the case, if the facts permitted, I should be well content to say with Pagenstecher, "Es darf also wohl unbedingt Hilton als der Entdecker der Trichinen gelten." I will admit that a legitimate doubt hangs over the claims of Tiedemann, whilst no doubt can be entertained that Hilton was the first to ascribe to these bodies an animal nature.

When it is asked, Who discovered Trichina? we must frame our reply according as to whether the inquirer refers to the worm itself, the disease which it superinduces, or the pathological appearances which its presence occasions. Summarily, my answer may be categorically expressed as follows:—

1. Mr. Paget first actually determined the existence of the entozoon which was subsequently more completely described by Professor Owen. Mr. Paget was assisted in the discovery by the celebrated botanist, Robert Brown, who lent his microscope for the purposes of examination.

2. Professor Owen first scientifically described and named the

flesh-worm *(Trichina spiralis)* in the published transactions of a learned Society. He first fully interpreted the true nematoid nature of the parasite.

3. Mr. Wormald had "more than once" previously noticed the characteristic specks "in subjects dissected at St. Bartholomew's" Hospital. He transmitted the individual specimens which enabled Owen to draw up his valuable paper.

4. Mr. Hilton was the first to suggest the parasitic and animal nature of the specks observed in human muscle. He anticipated Wormald in his observation of the "gritty" particles in dissecting-room subjects, and described the bodies as "probably depending upon the formation of very small cysticerci."

5. According to Dr. Hodgkin, "the first observation of these little bodies was made in 1828" by Mr. H. Peacock. The latter made a dried preparation of the *sterno-hyoideus* muscle to display the specks. That preparation is the oldest in existence, and may be seen in Guy's Museum.

6. Henle, Diesing, Küchenmeister, Davaine, Thudichum, and Aitken have pointed to a notice by Tiedemann as probably, or possibly, indicating a prior observation of the specks. Leuckart rejects the evidence. Pagenstecher appears to be in doubt as to the nature of the bodies in question. I have long entertained, and still entertain the persuasion that the passage in question gave a rough and imperfect description of the now familiarly known calcified Trichina capsules.

7. Herbst was the first to rear muscle-flesh-worms, or encapsuled Trichinæ in animals by experiment (1850); and Virchow was probably the first to rear and recognize sexually mature intestinal Trichinæ by experimenting upon a dog. (Deutsche Klinik, 1859, s. 430.)

8. Leuckart was the first to offer a full, complete, and correct solution of the principal questions relating to the source and mode of genesis of the flesh-worm (1860). He disproved the erroneous views of Küchenmeister.

9. Zenker opened up a new epoch in the history of trichinal discoveries. He first observed the young in the act of migration, and was the first to demonstrate that these parasites were capable of giving rise to a violent disease in the human body.

If any one of the above conclusions can be shown to be incorrect I shall be happy to abandon it. To myself, it is clear that Mr. Paget first discovered the worm. Owen first scientifically described it. Wormald transmitted the specimens to Owen, having himself previously seen similar calcified bodies. Hilton met with cases before Wormald, and first suggested the animal nature of the specks. Peacock anticipated Hilton in noticing the specks. Tiedemann probably anticipated Peacock.

By way of supplementing the first of the above conclusions, it only further remains for me to add that Mr. Paget speaks of Mr. Robert Brown as having himself "dexterously pulled a worm from a cyst." Mr. Brown was, therefore, the second person made acquainted with the parasite through Mr. Paget's instrumentality, as Mr. Children appears to have been the third. Portions of the trichinised muscle were afterwards "distributed far and wide," and it was not unnatural that specimens should immediately pass into the hands of Professor Owen, who, of all men then living, would be more likely than any one else to throw light on the structure and zoological affinities of the entozoon.

Those who desire yet fuller information than is here given, should read the original letters above referred to. To these documents (to which references will be found in the Supplemental Bibliography) I would especially direct the attention of foreign helminthologists—the more so, since there are persons, on this side the channel, who systematically ignore the labours of their own countrymen.

II.

RECORD OF SUCCESSFUL EXPERIMENTS WITH TRICHINA SPIRALIS.

In conducting these investigations I received the co-operation of Professor Simonds and Assistant-Professor Pritchard, of the Royal Veterinary College. The results were originally communicated by me to the Linnean Society:—

Exps. 1 and 2. On the 15th of March, 1865, an ounce of flesh containing Trichinæ was administered by myself to a black bitch. The dog being destroyed five days subsequently, neither intestinal nor muscle-trichinæ were discovered. It was thought that the dog had thrown up the bolus, which was strongly saturated with chloride-of-zinc solution. The bolus consisted of a portion of the *pectoralis major* of a subject brought to the dissecting-room at the Middlesex Hospital. The cysts were highly calcified; but the majority contained living embryos, which were quite unaffected by the zinc-solution injected into the body to prevent decomposition. At the same date a small white puppy was experimented on and examined with precisely the same results. In either case it was, of course, too early to expect muscle-flesh-worms to have become developed.

Exp. 3. Half an ounce of the same trichinous human flesh was given (at the same date) to a black-and-tan puppy reared at the Royal Veterinary College; a second "feeding" being administered on the 21st of March, or six days after the first. In this case Mr. Pritchard, who fed the animal, took the precaution to chop the muscle into small pieces and to mix it with other food, in order

that the flesh might be the more readily retained in the stomach. The puppy was not destroyed until the 15th of the following June, when, on examination, numerous encysted but non-calcified muscle-Trichinæ were found in all the voluntary muscles subjected to microscopic scrutiny.

Exp. 4. An ounce of the same flesh was given to a dark-coloured pig on the 15th of March, and again on the 20th, several other "feedings" being also administered during the month of April (1865). It was destroyed on the 16th of May; but no Trichinæ were detected.

Exp. 5. An ounce of the same human muscle-flesh administered to a small sheep (which was subsequently killed on the 29th of June) produced the same negative results.

Exp. 6 and 7. "Feedings" were at the same time administered to a rat and mouse. The mouse died on the 2nd April, when I examined its muscles without success. On the following day the rat unfortunately made its escape, but whether trichinized or no I cannot say.

Exp. 8. An ounce of trichinous human flesh was given to a donkey, in the form of "balls," on the 20th of March; and during the month of June four other separate "feedings" with trichinous dog's flesh were also administered. The animal was removed from the College without the result being ascertained.

Exp. 9. From the 15th to the 20th March, 1865, inclusive, three small Trichinæ "feedings" were likewise administered to a guinea-pig. This little animal was not destroyed until the 15th of the following June, when a positive result was obtained. The *pectoralis transversus* and other muscles were found to harbour a considerable number of encysted Trichinæ.

Exp. 10. On the 20th March, and again on the 21st (1865), "feedings" from the same human subject were administered to a hedgehog. On the 20th of April the animal seemed to be attacked with symptoms of Trichinosis. It refused food, kept its head extended and the eyelids closed. On the 27th it appeared much

worse, and on the morning of the 28th it was found dead. On the 29th I examined the flesh, and found abundance of living Trichinæ in the muscles. The capsules were very thin and transparent. A few days later Mr. Simonds also examined the flesh, and confirmed this result.

Exps. 11 and 12. Two chickens were fed, on the 21st of March, with the same material. One of the birds died on the 24th, when I examined the intestines and detected one or two very minute nematodes, which, at the time, I believed to be imperfectly developed Trichinæ, but subsequently saw reason to alter my opinion. The other bird died on the 3rd of April, and certainly contained no muscle-Trichinæ.

Exp. 13. On the 22nd and 23rd of March, "feedings" amounting to an ounce of flesh in all were given to a mole. This animal was returned to the care of Mr. Charles Land, who had previously sent it to the Veterinary College. He subsequently reported that, after observing the mole to be "working" for two or three days, he lost all trace of it, and concluded that it had either escaped or was dead.

Exp. 14. On the 1st and 2nd of May, portions of the left fore extremity of the hedgehog (in which we had successfully reared *Trichina* from the Middlesex-Hospital subject) were offered by Mr. Simonds to a cat. It ate the flesh very readily, consuming the entire limb. On the 15th of the following June the cat was killed, when living Trichinæ were found within all the muscles which we examined.

Exp. 15. At the same dates a young terrier dog was similarly treated, but did not take the "feeding" so readily. In this case the left hind extremity of the hedgehog was employed, and what was not eaten voluntarily was forcibly introduced. On the 1st of June the dog was attacked with "distemper," and died on the 8th of the same month. On examination we found several living Trichinæ in the *sterno-maxillaris* and other muscles. Some of the parasites were encysted.

Exp. 16. From the 9th to the 12th of June, inclusive, four separate worm feedings with the flesh ef the trichinized terrier-dog were administered to a crow. The bird was killed some months afterwards and sent to me for examination. Its muscles were entirely free from Trichinæ.

Exp. 17. From the 9th to the 17th of June, inclusive, seven separate worm-feedings were administered to a pig. One of the "feedings" was with the trichinized guinea-pig's flesh, the others from the dog. This animal was not destroyed until the 4th of April, 1866, when all the muscles which I examined were found extensively infested with Trichinæ. There were probably not less than 16,000,000 present, all being alive and enclosed within perfectly-formed capsules, none of which latter exhibited any traces of calcareous deposition.

Exp. 18. Four separate "feedings" with trichinous dog's-flesh were likewise, at the same dates as the foregoing, administered to a rat. This experimental animal, however, like the one previously mentioned, contrived to make its escape. I fear it was well trichinized.

Exp. 19. About the same date trichinous "feedings" were given to a black puppy (bred at the Veterinary College). The dog was killed on the 18th of August, 1866, having also been made the subject of an echinococcus-feeding, when I found abundance of encysted Trichinæ within the voluntary muscles.

Exp. 20. Four separate worm-feedings with the flesh of the trichinized guinea-pig were given to a sheep on the 15th, 16th, 17th, and 19th days of June, 1865. The experimental animal was destroyed on the 29th of the same month; but the result was negative.

Exps. 21 and 22. "Feedings" with the guinea-pig's flesh—four in the one case and three in the other—were also administered by Mr. Simonds (from the 15th to the 19th of June, inclusive) to a chicken and a goose respectively. These birds were destroyed some months afterwards and sent to me for examination; but the

most careful scrutiny failed to detect any Trichinæ within their muscles. The goose was cooked and eaten without the slightest hesitation. The chicken I found too tough for consumption.

Exp. 23. On the 28th of March of the present year, I obtained a small quantity of muscle from a highly trichinized German subject, who died, from the effects of an accident, at the London Hospital the day previous. The case was fully reported by Dr. Thudichum in a new journal, called "Scientific Opinion," (No. 4, April 25, 1866, p. 55). During the same day (at 2.30 P.M.) I fed a dog with part of this human flesh. On the morning of the 31st I killed the dog, and examined the intestinal canal (at 11.30 A.M.), which revealed the presence of sexually mature living Trichinæ. The males (of one of which I retain an accurate figure) displayed the characteristic bilobed caudal appendage, leaving no doubt as to their source and nature. I have mentioned the precise time of the experiment, in order to show that a period of sixty-nine hours proved amply sufficient for the development of the young muscle-flesh-worms of the human subject into the sexually mature adult Trichinæ of the dog.

Exp. 24. With another portion of this human flesh (taken from the muscles of the tongue) in which the Trichinæ were extraordinarily abundant, I fed a cat. In about ten days the animal showed the most marked symptoms of Trichinosis. It refused to eat; the eye lost its lustre; the body became very thin, and I thought the animal would die. By very great care, keeping it warm before the fire, and subsequently inducing it to take a little milk, the creature improved, gained flesh, and eventually recovered. About three months afterwards I destroyed this cat, when, on examining the *panniculus carnosus*, *latissimus dorsi*, and other superficial muscles, I found great quantities of well-developed, capsuled Trichinæ. Although the animal had swallowed scarcely a quarter of a ounce weight of infested flesh, yet thousands of parasites had been propagated in its flesh, and a nearly fatal helminthiasis set up. Dr. Thudichum, who saw the trichinized German

subject, estimated the number of parasites in his body at 40,000,000. I do not think this estimate likely to be exaggerated; for if all the flesh had been infested to the extent I found to obtain in respect of the muscles of the tongue, I believe 100,000,000 would have been tolerably near the mark. In places the point of a needle could not be thrust between the capsules, so closely were they agglomerated.

Exp. 25. From the 19th to the 25th of April, 1866, inclusive, daily administrations of trichinous pork, in the form of bolus, were given to a sheep by Mr. Pritchard. The Trichinæ were from one of our experimental animals at the Veterinary College, about two ounces of the flesh being taken at each feeding. The flesh of the sheep (destroyed in the following November) failed to give any indication of the presence of the offspring of these parasites.

Exps. 26 and 27. About the same time, and occasionally at intervals extending over a period of five weeks, Mr. Pritchard also fed two young fowls with the same trichinous pork. Towards the close of October, 1866, both birds died, when Mr. Pritchard carefully examined the flesh of them, but failed to find any trace of Trichinæ.

Exps. 28 and 29. From April 2nd to the 9th of the same month, 1866, inclusive, feedings with trichinous pork were likewise given to two more dogs. These animals were destroyed and examined by Mr. Pritchard in November, 1866; but the result appeared to be negative.

Remarks.—The results as here given correspond very closely with those obtained by investigators on the continent. Thus the seven experiments on birds (including five fowls, one goose, and one crow) were all negative. This conclusion, so far as muscle-Trichinæ are concerned, accords precisely with the experiences of Professors H. A. Pagenstecher and C. J. Fuchs, at the Zoological Institute in Heidelberg. These experimenters, it is true, found that the ingested muscle-Trichinæ acquired sexual maturity within the intestinal canal of their avian "hosts;" but they never found young

Trichinæ in the muscles of the birds, nor did they perceive any evidences of an attempt on the part of the escaped embryos to effect a wandering or active migration on their own account. Clearly, if the bird's intestinal canal were a proper territory for the residence of sexually mature Trichinæ, Drs. Pagenstecher and Fuchs would have found abundance of wandering and non-encapsuled flesh-worms, and we should have obtained (owing to the greater length of time which we generally allowed to elapse before destroying the experimental animals) sexually immature muscle-Trichinæ enclosed in well-formed capsules with, in some instances, more or less calcareous degeneration. I have put the matter thus pointedly, because not a few persons still entertain the notion that Trichinæ are liable to infest all kinds of warm-blooded, and even, also, many kinds of cold-blooded animals, such as reptiles and fishes. Certain nematodes found in earth-worms have been described as Trichinæ; and, consequently, pigs and hedgehogs were said to become trichinous through eating these annelids. The minute flesh-worms described by Bowman from the muscle of the eel are not true Trichinæ, any more than the somewhat similar parasites (*Myoryktes Weismanni*) which Eberth found to infest the muscles of the frog. The same may also be said of Dr. Salisbury's *Trichina cystica*, described in Dr. Hays' American Journal. (For ref. see Bibliography.) The negative results above obtained may therefore fairly be taken as positive, in one sense, inasmuch as they help us, with the aid of other experiences, to define the area of distribution legitimately assignable to *Trichina spiralis* as a good nematode species. Taking in connexion with what we know touching the limitations of distribution or occupation affecting other species of parasites, the facts have a special and very peculiar significance; one, however, upon which I cannot here enlarge. Deducting, therefore, from the twenty-nine experiments the seven instances among birds, where the flesh-worms would not develop themselves, and also the three separate cases where the experimental animals escaped, together with one other case where no

opportunity of examination was afforded, we have left us exactly eighteen mammals in which the results were, in all but two, very carefully ascertained. The exceptions were those of the first two dogs experimented on some two years back. The intestinal canal, or rather its mucous contents, were not sufficiently examined to enable me to affirm positively as to the absence of sexually mature Trichinæ. In a more recent instance (Exp. No. 23) I had evidence of the difficulty of finding the mature Trichinæ in the mucous and half-digested intestinal contents, although the experiment eventually proved perfectly successful. Of course the difficulty of testing the result is a thousandfold increased where only a very small number of Trichinæ have been administered. In the sixteen remaining cases the results appeared to have been fully ascertained; and out of these, nine were perfectly successful. The "negatives" comprised three sheep, two dogs, one pig, and a mouse. The "positives" embraced four dogs, two cats, one pig, one guinea-pig, and a hedgehog. At all events, at least one-half of the experiments on mammals yielded positive results, which, considering all the circumstances of the case, is by no means unsatisfactory. Carnivorous mammals, and especially those which subsist on a mixed diet, appear to be most liable to entertain Trichinæ; nevertheless it is quite possible to rear flesh-worms in herbivora. Pagenstecher and Fuchs succeeded in rearing muscle-Trichinæ in a calf; and they found three female intestinal Trichinæ in a goat, but, apparently no muscle-flesh-worms, although twenty-seven days had elapsed since the first feeding with trichinized rabbit's flesh. In our three sheep no trace of Trichinæ could be found. However, on account of the expense, comparatively few experiments have been made on herbivora; and, therefore, perhaps, it is as well not to speak too positively from the data already afforded. There is really no practical need for any further experiments with this species of parasite; but it is quite clear that, in their natural state, herbivorous mammals can seldom have an opportunity of infesting themselves, whilst the reverse is the case with swine,

carnivorous mammals, and ourselves. Because many quadrupeds may become trichinous, it does not follow that all mammals are liable to be infested. In the case of other parasites (as with the common fluke) we find them limited to a larger or smaller number of hosts; whilst, on the other hand, in not a few cases, the territory occupied is that of the body of a single species. On this subject I need not dwell; but I may instance as examples of very limited distribution the two most common cestodes liable to infest the human body. Nematodes, again, display analogous peculiarities of distribution; but, looking at the subject in relation to the public health, I have no hesitation in saying that a great deal of unnecessary fear has been created in this couutry. No doubt the Imperial authority in Russia had good grounds for recently issuing an order prohibiting the importation of pork into that country, since severe endemics of Trichiniasis had occurred in neighbouring German states. In this country, however, ordinary precautions will suffice. English swine are almost entirely, if not absolutely, free from this so-called disease; and not a single case of Trichiniasis in the living human subject has been diagnosed in the United Kingdom. Some twenty or thirty cases have been discovered *post mortem*; and it is highly probable that most, if not all, of these trichinised individuals had contracted the disease, during life, by eating German pork sausages or other preparations of foreign meat.

III.

ON THE REARING OF THE LARVÆ OF TÆNIA MEDIOCANELLATA, BY EXPERIMENT WITH "PROGLOTTIDES."

I ORIGINALLY drew up the following observations which were afterward submitted by Professor Simonds and myself, as a joint communication, to the Royal Society. The experiment here fully described was the first of the kind performed in this country, having been anticipated only by Professors Leuckart and Mosler; the former being the first to rear Cystic Entozoa in the calf, thus producing the diseased phenomena, which he called the "acute cestode tuberculosis."

Neither of us having exhausted certain funds placed at our disposal for scientific purposes (in the one case by the Royal Agricultural Society through the Governors of the Royal Veterinary College, and in the other by the British Association for the Advancement of Science), we united the resources which severally remained to us, and instituted a series of practical experiments in helminthology. The investigations have been, for the most part, eminently successful.

The subject selected for this experiment was a fine healthy female calf, about a month old, living at the time on the milk of its dam. As we were unable to obtain possession of the dam, another cow was procured as a foster-mother, and the calf was placed with her in order that it might receive a proper supply of milk in the natural way. This plan was preferred to that of obtaining a weaned calf, as being better cal-

culated to preserve the health and strength of the young animal. In the course of a few days the two animals became perfectly accustomed to each other, the calf taking nourishment as often as was requisite.

On the 21st of December, 1864, we administered to the calf eighty mature proglottides of the *Tænia mediocanellata*, mingled with a little warm milk in the form of a draught. The potion was taken readily, and the worm-joints probably entered the stomach in a perfect and unbroken condition. No alteration was made in the subsequent management of the animals, but a careful daily watch was kept upon the calf.

For some time no indications were perceived of disturbed health; but on the 6th of January, 1865 (the sixteenth day after the experiment), a careful observation showed that the animal, although lively (and taking its milk, and likewise some hay with undiminished appetite), was nevertheless suffering from some persistent cause of irritation. It would often be nibbling at its legs and other parts of its body, and trying with its mouth and tongue to get at places which were beyond its ordinary reach. It would also frequently rub itself against the manger and sides of the loose box in which it was confined. Desisting from this, it would arch its spine and stretch out its hind limbs in an altogether unusual manner. It would also strain itself repeatedly, at such times voiding either urine or fæces, or occasionally both in small quantity. There was, however, no expression of suffering in the countenance, no disturbance of the breathing or of the circulation, no injection of the visible mucous membranes, no alteration of the temperature of the body, no "staring" of the coat, nor rigors; in short, no indication of anything seriously wrong. These symptoms continued throughout the next day with little variation; on the third day they had nearly passed away, and by the fourth had entirely disappeared.

On the 25th of January, 1865, just five weeks after the first worm-feeding, two hundred more of the mature proglottides of

Tænia mediocanellata were administered; but one hundred of these worm-segments had been previously immersed in a weak alcoholic solution, strong enough, it was feared, to destroy the vitality of their contained eggs. The other hundred proglottides were in beautiful condition, and for the most part appeared to be thoroughly mature. Again the calf took the feeding readily, and little or no force had to be employed in holding it during the administration. However, directly on being loosed, it was observed to show some symptoms of distress in the breathing, accompanied with trembling. The feeding took place at 3 P.M., and as the night promised to be cold, it was placed with the cow in a closed and warm stable. On the following morning, it was noticed that the tremors had somewhat abated, but the animal was evidently dispirited, and would every now and then grind its teeth as if in pain. Its appetite was much diminished. By the next day, however, all these diseased symptoms passed away, and the animal recovered its ordinary healthy aspect.

On the 1st of February, the seventh day succeeding the second worm-feeding, there was a decided return of the nervous irritability; but in a day or two these symptoms again declined. Nevertheless the animal was not quite right; the coat began to lose its natural and glossy appearance, and there was an evident loss of flesh.

Feb. 8th.—A marked change for the worse has taken place. The animal is dull and dispirited; refuses all food excepting milk, and of this takes but little; it arches the back frequently, and stretches the limbs in a peculiar manner; the breathing and the pulse have increased, and at intervals slight tremors are observable, more particularly of the muscles of the neck and shoulders.

Feb. 9th.—All the unhealthy symptoms are more marked. The pulse numbers 120, and the breathing 35 in the minute. The tremors are more continuous, and the condition of the animal is fast declining.

Feb. 10th.—Still worse. The calf is so ill that we fear a

fatal result. It takes little or no notice of the cow, and cannot be induced to suck. The eyes have a peculiar staring expression.

Feb. 11th.—The severity of the symptoms has somewat abated this morning. The animal is rather more lively, and will now and then take a little milk. The breathing and pulse, however, remain rapid. The tremors, though still frequent, have diminished in intensity. Towards the after part of the day the improvement became more marked; therefore, instead of destroying the animal (as we had purposed in the event of its becoming much worse), we resolved to satisfy ourselves, by other means, as to whether the above symptoms were really due to parasite-invasion. Accordingly, a small portion of the right sterno-maxillaris muscle was removed by operation, and this fragment of the flesh, although weighing only twenty-two grains, revealed the presence of three imperfectly developed cysticercus-vesicles. Each was about the size of a pin's head, but they displayed no trace of calcareous corpuscles, or of cephalic formation in their interior. On the assumption (afterwards, however, found to be erroneous) that all the muscles of the body might be similarly affected, and to the same extent, it was at the time calculated that the animal "entertained" some 30,000 of these artificially introduced "guests."

Feb. 12th.—A further improvement has taken place, but the animal is still dispirited, the pulse and breathing continuing abnormally rapid. The eyes are less staring.

Feb. 13th.—Improvement continues; breathing less rapid; the tremors have disappeared.

Feb. 15th.—Pulse diminishing; breathing nearly normal; appetite good.

Feb. 22nd.—Convalescence perfectly re-established.

Throughout the remainder of the month of February, and during the whole of March, the calf continued to maintain complete vigour, and, indeed, gained flesh so rapidly, that at the beginning of April it might readily have been sold to a farmer, to

a butcher, or to a cattle-dealer, as a thoroughly sound and thriving young beast. The time having, however, arrived for determining the result of the experiment, the calf was slaughtered on the 3rd of April, by division of the right carotid artery. The operation was performed by Mr. Pritchard, who also, during the subsequent *post-mortem* examination, rendered us essential service. As before, so after its death, all present remarked the particularly healthy aspect of the animal, there being no external indications by which the most practised professional eye could have discovered the existence of internal disease. But for our previous trial, we should ourselves have been doubtful of finding any Entozoa within the flesh.

Immediately after the first incision along the median line of the thorax, a solitary Cysticercus came into view, many others successively displaying themselves as the integument was being raised and dissected from off the left side of the carcase. No person in this country having hitherto witnessed such a demonstration as now followed, I may perhaps be permitted to express the natural feeling of astonishment which all shared on thus beholding hundreds of larval cestode parasites in the flesh of an animal not usually considered capable of harbouring "measles" after the fashion of swine.

Examined individually, the larvæ were enclosed in oval sacs, whose transparency permitted us to see, at or near the centre of each vesicle, internally, a minute white spot, representing the so-called receptaculum capitis. On subsequent rupture of the cyst, a microscopic examination of the contained larva revealed the ordinary characters of the Cysticercus which produces the *Tænia mediocanellata*.

Speaking generally, it may be said that the connective tissue and cellular aponeuroses were very feebly invaded; but in certain situations, such as those occupied by the linea semicircularis and fascia lumbaris, several vesicles were closely associated; moreover, as regards the muscles themselves, extensive parasitic invasion was

prevalent only in the more superficial layers. It was likewise noticed, as obtains in the parallel case of Trichina, that the larvæ were disposed in the longitudinal direction of the muscular fibres, being at the same time more numerously grouped towards the points of osseous insertion or of aponeurotic attachment. Not a few large vesicles had inflamed and suppurated, the cysts being occupied internally by a thick green-coloured deposit.

Referring to the left side only, we noted that all the breast-muscles (pectoralis major, p. transversus, and p. anticus) were much infested, but scarcely so fully as the more superficial panniculus carnosus. In the latissimus dorsi and trapezius the cysts were very numerous, rather less so in the combined levator humeri and sterno-occipitalis, somewhat fewer in the rhomboideus brevis and rhomboideus longus, and exceedingly scanty in the superior part of the scalenus, the remainder of this last-named muscle being entirely free. The lateralis sterni contained none; neither were any observed in the abdominal region of the serratus magnus, but several vesicles were lodged in the superficial cervical portion of this muscle. Not a few existed in the upper part of the complexus major and in the minor, some also occurring in the longissimus dorsi; yet none were observed in the spinalis dorsi, in the superficialis costalis, or in the diaphragm.

Turning towards the neck-region, we found them abundant in the sterno-maxillaris, considerably less so in the splenius, only one in the hyoideus, several in the sterno-hyo-thryoideus, but none in the longus colli. All the other deep-seated muscles of this region, including the obliquus capitis superior and inferior, as well as the rectus capitis posticus major and minor, appeared free from any trace of the vesicles. On the other hand, all the superficial muscles of the face, such as the retractor anguli oris, orbicularis oris, and levator palpebrarum, gave abundant evidence of their presence, the vesicles being particularly numerous at the outer part of the massetus externus. In like manner their presence was only less strongly indicated in the muscles of the eyeball, such as the

obliquus inferior, adductor and retractor oculi, also in the depressor oculi, one "measle" being placed between the tendon of this last-named muscle and the sclerotic coat. The ball of the eye contained no vesicles. A few were remarked in the substance of the genio-hyoideus and other muscles supplying the tongue; but the lingual organ itself appeared to be entirely free.

As regards the anterior extremity, we found the Cysticerci very numerous in the teres externus and abductor humeralis, being scarcely less abundant in the spinatus anticus and posticus. They were likewise prevalent in the front part of the triceps extensor brachii, but altogether wanting behind and in the deeper portions of this muscle. A few were remarked in the flexor brachii, whilst the subscapularis, teres internus, and coraco-humeralis failed to reveal any. They were very abundant in the flexor metacarpi externus, less so in the flexor metacarpi medius, and comparatively scanty in the flexor metacarpi internus. The lower part of the combined flexor perforatus and perforans shewed a few, several being likewise present in the accessorius ulnaris. They were rather more abundant in the extensor metacarpi magnus, also in the extensor et adductor digitorum, likewise in the extensor digiti externus, and scarcely less so in the extensor metacarpi obliquus; yet none could be discovered either in the anconeus or in the humeralis externus.

Over the haunch, and throughout the surface-flesh of the left hinder limb, the Cysticerci were particularly abundant, being numerous in the gluteus maximus, in the tensor vaginæ femoris, and most especially in the large triceps abductor femoris. They were little less abundant in the vastus externus, and in those limited portions of the gastrocnemius externus and internus which come near the surface. A few vesicles were observed at the subcutaneous posterior section of the ischio-tibialis, also in the outer part of the biceps rotator tibialis and rectus femoris; yet none were noticed either in the gluteus internus and gracilis, or in the vastus internus and sartorius. In the flexor metatarsi and exten-

sor pedis they were rather numerous, but, at the same time, comparatively scarce in the peroneus and flexor pedis perforans. Lastly, none were detected in either the psoas magnus or psoas parvus.

With the exception of the heart, none of the viscera showed Cysticerci, the lungs, liver, kidneys, spleen, and thymus gland being absolutely free; neither were any discovered in the brain. In short, it may be stated that the internal organs of the body generally were perfectly healthy; and even as regards the heart itself, the rather numerous vesicles found there displayed only a very incomplete development. At first they looked as if they might belong to a separate swarm-brood; but a careful microscopic examination disproved this notion, and, at the same time, revealed some curious facts. In the heart none of the vesicles had attained one-third of the size of those prevalent in the muscles, yet their age was doubtless the same; for although none of those examined displayed a well-formed head with the characteristic and normal number of suckers, yet one vesicle was found to possess three suckers, another having two suckers, and a third only a single sucker (see Fig. 1, p. 28). Most of the vesicles were entirely suckerless, whilst those which had them showed other indications of abnormality. The suckers themselves were not perfectly formed, in most cases, and there were commencing signs of calcareous degeneration. In some instances, the entire contents of the vesicles appeared to have been absorbed, leaving only faint white spots to indicate the situations where the cysts once were. Such, at least, is my interpretation of the phenomena observed; and, in this relation, I have further to remark that the heart-cysts were not merely found at the surface of the organ, but were dispersed throughout its substance, one or two of the better-formed vesicles being lodged within the septum ventriculorum.

Only three experiments of this kind have been previously performed on the calf; namely, two by Leuckart, and one

by Mosler. In two of these instances the experimental animal perished, whilst in the other case, as in our own, the creature barely escaped with its life. To our animal we administered a larger number of proglottides than had been given even in Mosler's case; but, probably in consequence of the embryonic immaturity of the contents of many of the eggs, we did not get that fatal result which otherwise would inevitably have followed from a larger migration of the cestode-progeny. We believe that by far the greater proportion of the "measles" resulted from the second worm-feeding, in which case they would have come from the hundred proglottides not subjected to the action of alcohol. Although the characters presented by the earlier developed morbid symptoms, as well as the time of their accession, induce us to attribute the diseased phenomena to the larvæ set free by the first "feeding," yet it is clear, from the feebleness of the symptoms manifested, that only a very inconsiderable number of embryos can at that time have entered on their wanderings. In the second "feeding," however, the case is very different; for here all the circumstances connected with the subsequent marked disturbance of the animal's health point unequivocally to the development of that peculiar form of parasite-disease designated by Leuckart as the "acute cestode tuberculosis."

From the number of young vesicles present in the minute portion of muscle removed by operation from the living animal, I had publicly announced my belief that we might ultimately find 30,000 Cysticerci developed in this calf; but as the larvæ were afterwards found to be almost entirely confined to the superficial muscular layers, it turned out that the calculation was considerably beyond the mark. Nevertheless, from *post-mortem* data, we calculated that there were from seven to eight thousand "measles" present, 130 vesicles having been counted at the surface of a single muscle.

IV.

FURTHER OBSERVATIONS ON THE LARVÆ OF TÆNIA MEDIOCANELLATA, WITH REMARKS ON CYSTICERCI FROM MUTTON.

In the foregoing observations I have made allusion to the incomplete development of the measles in the heart of the calf which formed the subject of our first experiment. The facts noticed at the time of dissection were so peculiar, that I purposely reserved a more detailed account of them for some future occasion. Up to the present time, however, my notes on this head, together with many others, have never been made use of; and though, even now in a necessarily fragmentary condition, they are, I think, sufficiently important to be placed on record.

According to my MS., the left side of the heart of the calf displayed about twenty rudimentary measles, not one of which had advanced in development sufficiently to show any trace of head or sucker. Deeper within the substance of the wall of the left ventricle, and in the septum, similar abortive formations were rather more developed. Thus one otherwise well-formed but small vesicle, measuring $\frac{1}{10}$″ from the upper part of the septum, showed a minute calcareous particle in its centre; but there was no sucker. In another similar cyst there were two irregularly-rounded concretions, indicating the situations where two suckers had commenced developing. One of the most perfectly developed of these abortive formations was the Cysticercus which I have here drawn (Fig. 1). It was obtained from the substance of the wall of the left ventricle; the "measle," divested of its cyst,

measuring about the $\frac{1}{11}''$. In this case there was one tolerably well-developed sucker (*a*), whose cavity was partly occupied by calcareous matter (*b*). The diameter of the sucker was less than $\frac{1}{30}''$. There were indications of two other suckers, with corresponding patches of calcareous deposition (*c*, *c*). The ordinary corpuscles (*d*, *d*), which I have elsewhere termed "sclerous" particles (to distinguish them from morbid deposits), were as abundant as they usually are in healthy measles. The so-called neck of the Cysticercus (*e*) was tolerably well-formed, its characteristic transverse foldings being well seen through the delicate, transparent, and closely applied caudal vesicle, which, as usual,

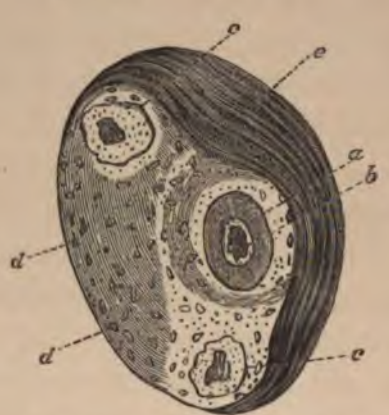

FIG. 1.—Imperfectly developed "Beef-measle" from the heart of a Calf. (× 30 diam.)—Original.

enclosed all the other structures. I have recorded these additional facts, not merely on account of the pathological significance which they collectively present, but in order to contrast them with the results which I obtained in another experiment, where the natural process of cure was observed.

Where the measle has once attained its perfect development, its subsequent calcareous degeneration is a comparatively slow process, and the calcareous mass resulting is naturally larger and more compact. In the 17th Volume of the "Transactions of the Pathological Society" (p. 464), I have described these appearances with tolerable minuteness; but, in a few words, it may be

said that, under such circumstances (as those described at p. 34), we only find each separate calcified mass closely invested by its sheath, which latter is partly made up of the original cyst and partly of the so-called connective tissue. As I have been frequently asked to explain the cause of the imperfect development of measles in the heart, I may state, at once, that the subject is involved in much difficulty. I have hitherto supposed that the density of the muscular fibres of the heart, as contrasted with that of the ordinary voluntary muscles, might be sufficient to explain it; whilst the constant and powerful contraction of the organ would certainly tend to restrict the expansion of the young delicate Cysticercus. I still think this the main cause; but, unfortunately, there are instances where the cestode larvæ have entirely overcome this pressure, and have attained a full development. Instances are on record where large hydatids have formed within the muscular substance of this organ in man, and also, I believe, in the lower animals; at all events, the museum of Guy's Hospital contains the heart of a bear which is extensively occupied with Cysticerci. It would be extremely interesting to ascertain to what species of tapeworm these larvæ in the bear are referable. The specimen is a very remarkable one, and apparently unique. I believe there is no published description of it on record. In this connection it is also interesting to keep in mind that small nematodes as, for example, Trichinæ, though they enter the substance of the heart, will not rest and develope there; but many of the larger kinds (species of Ascaris and Prosthecosacter) will freely take up their residence within the ventricles, and attain individually a considerable size. The subject is worthy of further investigation, from whatever point we may choose to view it; but the facts at present made known are too scanty to enable us to arrive at any very definite conclusions.

For convenience sake, I think it desirable to distinguish the larvæ of our various cestodes by different names. Thus, I have been in the habit of recognizing the above described larva under

the name of *Cysticercus bovis*, or the "veal and beef measle." This distinguishes it from the more commonly known *Cysticercus* (telæ) *cellulosæ*, or "pork measle," and also from a third measle, which I believe to be distinct, and to which I have given the name of *Cysticercus ovis*, or "mutton-measle" (Fig. 2). It would, undoubtedly, be also better to recognize the pork-measle under the title *Cysticercus suis*.

In the Appendix to my small work on the "Tapeworms" liable to infest the human body, and in the 18th Vol. of the

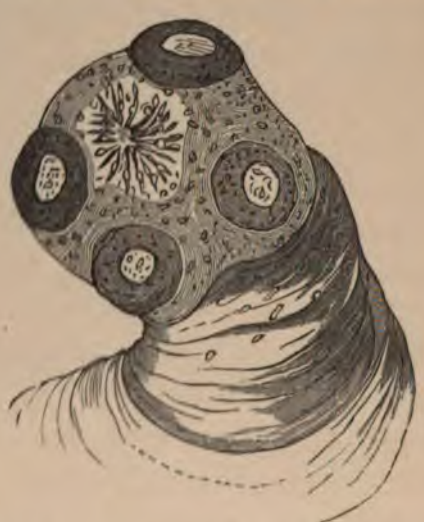

FIG. 2.—Head and neck of a "measle" removed from the centre of a mutton chop. Mr. Heisch's specimen. (× 40 diam.)—Original.

"Transactions of the Pathological Society," I have referred, without particular description, to the existence of this probably distinct form of cestode larva derived from mutton. Since the parasite may acquire more importance when better known, I have here, provisionally, at least, given it the above distinctive name; which, with the few additional details here offered, may lead to the determination of the probable source of its adult representative. The measle having been removed from its cyst, and the caudal vesicle being detached before I had an opportunity of inspecting the specimen, its size could not be accurately ascertained. Probably

it was much smaller than ordinary pork-cysticercus. The head measures $\frac{1}{30}''$ in breadth, and is armed with a double crown of hooks, twenty-six in number, the larger individually measuring $\frac{1}{100}''$. The suckers are four in number, having severally a breadth of $\frac{1}{100}''$. The so-called neck (as well as the head) is abundantly supplied with calcareous corpuscles, being at the same time marked by several transverse rugæ.

On three separate occasions I have noticed small hydatigenous cysts in joints of mutton brought to my own table. Dr. Kirk informs me that he has observed similar appearances. My specimens were all calcified; but the one received by me from Mr. Heisch was, as may be seen by the accompanying figure, an almost perfect example of an armed Cysticercus.

In this connection I may mention that I recently received from Dr. Prior part of a purchased Westphalian ham which was literally swarming with "measles." From a portion, weighing about three drachms, I removed upwards of twenty Cysticerci. The measles appear to have been pretty equably distributed throughout the ham; nevertheless, I understood that the tradesman who sold it did not scruple to eat a hearty meal off this diseased meat in order to satisfy his customers as to its value. Of course, if well cooked, no harm could possibly result; nevertheless, the mere exhibition of such infested flesh should be sufficient to excite disgust in the minds of most people.

V.

ADDITIONAL SUCCESSFUL EXPERIMENTS RESPECTING TÆNIA MEDIOCANELLATA, T. SERRATA, T. MARGINATA, AND T. CŒNURUS.

THE following experiments may be regarded as a continuation of the foregoing. The procuring and selecting of the parasitic materials usually devolved upon myself; whilst, on the other hand, Professors Simonds and Pritchard commonly performed the administrations. Other persons frequently rendered assistance during the "feedings." Mr. Simonds also conducted certain experiments independently; but only those for which I may be considered as exclusively responsible are here enumerated and described. A second calf was experimented on, but as I had not an opportunity of examining its flesh myself, I am by no means satisfied that the results were negative. The animal, I was informed, died from an attack of rinderpest about six months after the first "feedings" were administered.

The second successful case in which I took part was that in which a fine healthy Dutch heifer formed the subject of experiment. On the 3rd of March, 1865, this animal was made to swallow ninety proglottides of *Tænia mediocanellata*, given in tepid water. On the 15th of the same month, 108 "joints" were also administered. On the 5th of April, another hundred segments were conveyed to the stomach; and, for a few days subsequent to this last feeding, the animal became rather restless, bellowing occasionally. As no other symptoms followed, I made, on the 13th of April, a very careful selection of 200 ripe proglottides from tapeworms which

I had only that very day received from Dr. Fleming, of Birmingham. During several succeeding days the restlessness appeared to increase slightly, and the bellowing likewise continued. Moreover, there were certain peculiarities of manner which convinced Mr. Simonds (who watched the animal almost daily) that we had here, at least, a feeble development of those symptoms of the "acute cestode tuberculosis" which were so strongly marked in our first experiment on a calf. There was the same vacant stare, a dull expression of the eye, slight arching of the back, and stretching the limbs. However, the heifer never lost her appetite; and, strange to say, whilst all the cattle near her were attacked by (and most of them died of) the rinderpest, she entirely resisted its invasion. In about a week all the measle-symptoms passed off, and in course of time the animal attained the proportions and aspect of a large, healthy, three-year old cow. It was not until the expiration of rather more than a year from the time of our first feedings, namely, on the 4th of April, 1866, that the beast was slaughtered. With Mr. Pritchard's assistance, I made a very careful dissection and examination of the beef thus obtained; nor was it until I had subjected the muscles for many minutes to a very close and careful scrutiny (with the aid of a pocket-lens) that I succeeded in detecting evidence of the successful character of our experiment. The result was most interesting, especially since none of the hitherto recorded experiments (either by ourselves, or those previously instituted by Leuckart and Mosler) had extended over such a period of time. Here the only indication of the presence of measles was shown by the existence of extremely minute calcareous specks, larger, it is true, but by no means so conspicuous as ordinary Trichina-capsules. They presented the aspect of sparsely-scattered yellowish points, without any definite outline, or any other character likely to catch the eye. It is even difficult to see them in sections of the flesh which I have preserved in order to show them. Microscopic examination affords evidence of the remains of a small cyst; but all trace of the measle

itself is entirely lost. Each Cysticercus had become, as it were, resolved into a more or less amorphous, solid, friable, crystalline particle, whose true characters (as presented to the naked eye) were obscured by a closely investing and remarkably contracted cyst, the external surface of the latter becoming insensibly blended with the ordinary intermuscular connective tissue. By a little care, however, the calcareous particles could be perfectly isolated from their cysts. They were numerous, and, owing to the deeper colour of the muscular substance, most easily found in the diaphragm. In other muscles they were, perhaps, equally abundant, but it was a great labour to find them. Assuming them to have been pretty equably distributed throughout the muscular system, there could not have been less than 12,000 of these degenerated measles in the animal. I have no hesitation in saying that, if there had been 12,000,000 of these measles, no butcher would ever have noticed them; but, fortunately, in this condition their ingestion could do no possible harm. In point of fact, the flesh of the animal has since been entirely eaten; and healthier beef I never saw. Even in their perfect, non-degenerated state, the beef measles are readily overlooked; and, until recently, no person besides Mr. Simonds, Mr. Pritchard, and myself, and those who assisted us, had ever seen them in this country. The experiment now recorded is the first in which the natural process of cure has been traced; and it is of great practical importance, inasmuch as it proves to demonstration that a period of ten or twelve months is fully sufficient to ensure the natural death (by calcareous degeneration) of the smaller tapeworm-larvæ which reside in cattle. This is a positive contribution to our knowledge of the life-economy of these smaller Cysticerci, and it serves to fix the period of their larval activity. All the Entozoa, in their juvenile stages, whilst occupying the flesh of man and animals, are liable to be affected by this law of calcareous degeneration; but the actual time required to bring about the death of the parasite varies considerably in different species.

Tænia serrata.—Exp. 1. Three examples of the pea-shaped hydatid (*Cysticercus pisiformis*) were removed from the abdominal viscera of a recently killed rabbit. Two of them were rather more perfectly developed and larger than the third. The rabbit harboured no others. These three cestode larvæ were administered, on the 25th of January, 1865, to a healthy puppy, reared at the Veterinary College, and nearly twelve weeks old. Five days subsequently, namely, on the 30th of the same month, the puppy was destroyed. In the alimentary canal I found three young *Tæniæ*, and three only. Two of them were severally about one inch in length, the third being a trifle shorter, and not quite so large. These sexually immature tapeworms presented all the true characteristics of *T. serrata*, and their degree of development was in exact accordance with my previous experiences in this relation. The more feeble development of the third tapeworm proved its genetic connection with that larva which, at the time of the worm-administration, was noticed and recorded as being rather incompletely developed. The experiment was a perfect success.

Exp. 2. On the 25th of January, 1865, a second rabbit was destroyed, for the purpose of procuring additional tapeworm-larvæ. In this rabbit I only found one solitary and perfect *Cysticercus pisiformis*—a somewhat unusual circumstance. Mr. Simonds placed this larva in a small piece of paper, and, in the form of a bolus, gave it to a little puppy which belonged to the same litter as the former, being readily distinguished by its black ears. We did not destroy this whelp until the 7th of the following February, thus allowing a period of thirteen days to elapse for the development of the solitary *Tænia serrata* which we desired to rear from the Cysticercus. Mr. Simonds, as usual, requested me to examine the alimentary canal; and I had the satisfaction of removing the solitary *Tænia serrata* we had thus successfully reared. It was just six inches in length, showing the same correspondency as to growth, in respect of time, which my previous and independent experiences with the artificial method of rearing this parasite had

invariably supplied. The proglottides were still immature, but the cephalic development was in all respects complete.

Tænia marginata.—On the 28th of January, 1865, Mr. Simonds removed fourteen specimens of the so-called slender-necked hydatid from the omentum and surrounding viscera of a hogget. Five of these large Cysticerci (*C. tenuicollis*) were administered to the mother of the two whelps above referred to. They were swallowed entire, without any additional material. This dog was killed on the 7th of the following February, thus allowing only ten days for the development of the corresponding adult tape-worms. As usually happens in all old dogs, we found in the intestinal passages many examples of the extremely common cucumerine tapeworm (*T. cucumerina*); but these, of course, could have no genetic relation to the tapeworm-larvæ, which latter were already known to be the offspring of an entirely different species of cestode parasite. Our experiment, indeed, was a perfect success; for, in addition to the cucumerine tapeworms (of which there were fifteen mature specimens of variable size and growth), I found in the duodenal portion of the intestinal canal five young and sexually immature examples of the *Tænia marginata.* These were of uniform size, and severally one inch in length. Not a shadow of doubt could be entertained as to their relation to the five Cysticerci which had been administered. Their specific characteristics, uniform development, degree of growth, and separate location in the canal, pointed unequivocally to the source whence they had been derived. Here again, therefore, previous experiences received abundant confirmation.

Tænia cœnurus.—Exp. 1. On the 15th of March, 1865, Mr. Simonds received (from Messrs. Caudwell, of St. Neots) the head of a "giddy" sheep which had been killed two days previously. On examination the brain was found to contain a perfect polycephalous hydatid *(Cœnurus cerebralis)* which, it was estimated, supported about 150 scoleciform processes, or larval tape-worm-heads. It was removed entire, and given by Mr. Simonds to a

dog, which swallowed the parasite readily in its unbroken condition. Only five days subsequently, namely on the 20th of March, the experimental animal was destroyed; and here again a complete success attended our experiment. A careful examination of the alimentary mucus enabled me to extract a large number of extremely minute tapeworms, of so small a size that they were scarcely visible to the naked eye. Though only the heads and necks were developed, their characters corresponded with what was hitherto known of the head and neck of *Tænia cœnurus*, to which species they were undoubtedly referable. Their number appeared to correspond precisely with the number of scoleces attached to the common "gid" vesicle; but no attempt was made to collect every specimen, as that would have involved an immense amount of unnecessary labour. All trace of the vesicle common to the colony of "heads" had disappeared, the necks of the scolices, detached by digestive action, having become rounded off and more or less pointed (according to the degree of contraction shown by each specimen while under microscopic examination).

Exp. 2. On the 6th of April, 1865, a similar administration, with a brain-hydatid furnished with about 100 cephalic processes, was performed on a stray dog. Two days subsequently, however, the animal was claimed by its rightful owner, and we had therefore, in this instance, no opportunity of ascertaining the result of our experiment.

Exp. 3. On the 25th of April, Mr. Simonds received (from Mr. Mackinder, of Peterborough) the head of a sheep affected with "gid." The brain was found to contain three *T. cœnuri*. One of these, furnished with numerous heads, was given to a large half-bred lurcher dog. On the 16th of the following May, the animal was destroyed, thus permitting twenty-one days for the development of the slow-growing *Tænia cœnurus*. As too often happens in the case of old dogs, the digestive passages were found loaded with a great variety of tapeworms; nevertheless it was here again quite easy for us to distinguish between the parasites which had

been introduced by our experiment and those which had gained access to the canine "bearer" without our aid. Thus there were found in this "lurcher" several examples of the large *Tænia marginata*, six or eight specimens of *Tænia serrata*, and a few examples also of *T. cucumerina*. There were likewise some half-dozen nematodes *(Strongyli)*. In addition to all these, however, there were a multitude of small tapeworms (of the species *T. cœnurus*), the longest of which did not exceed one inch and a half in length, sexually immature, and manifestly corresponding with the numerous scolices artificially introduced. Again, therefore, despite the inconveniences always liable to attend administrations of this kind on old dogs, we had abundant proof of the success of our experiment.

Exp. 4. On the 25th of April, 1865, another of the two remaining *T. cœnuri* just mentioned was given by Mr. Simonds to a smaller dog. This hydatid was also well furnished with cephalic processes (or larval tapeworm-heads). In this case the experimental animal was not killed until more than two months had elapsed from the time of the worm-administration. On the 29th of the following June we made the necessary examination. Again we were successful. The intestinal canal contained large numbers of the *Tænia cœnurus*, there being no other kinds of Entozoa present. The experiment was therefore even more satisfactory than the previous one. The largest specimens measured about eighteen inches in length; but when I examined their proglottides, the contained eggs were still not quite perfectly developed. The primitive yelk-sacs and superfluous granular yelk masses were very conspicuous, but the true chorional envelope was only in the act of forming. The swallowing of the ova in this imperfect stage would lead to no result. Probably another week or ten days would have sufficed to render all these *Tæniæ* sexually mature.

Exp. 5. On the 8th of July, 1865, a large *T. cœnurus* was given to a terrier, which was also subsequently fed with trichinous flesh.

This animal died on the 2nd of August of the following year, but unfortunately neither Mr. Simonds nor myself had any opportunity of examining the contents of its viscera. A portion of its muscle-flesh was sent to me for microscopic examination, and this I found to contain encysted *Trichinæ*.

In addition to the above, several other experiments were performed with the larvæ of *Tænia solium* and *T. echinococcus*. In these instances we were unsuccessful. The same negative result followed the attempts to rear the larvæ of *Tænia mediocanellata* in the sheep. It is not impossible that the armed Cysticerci which I have described as occurring in mutton may turn out to be referable to a species of cestode infesting the higher carnivora; nevertheless, I entertain a well-grounded suspicion that the human "host" is liable, not only to entertain the beef and pork tapeworms, but likewise a third form derived from mutton. I believe the smaller varieties of the so-called *Tænia solium* answer to this hitherto unrecognized cestode.

VI.

ON THE NATURE OF PSEUDENTOZOA FOUND IN DISEASED AND HEALTHY CATTLE.

In the year 1865 the public mind of this country was thoroughly roused to a sense of the dangers consequent upon eating diseased meat. The panic arose chiefly from the severe outbreaks of Trichiniasis previously reported from Germany. During the excitement which prevailed at the time of the rinderpest, all sorts of erroneous notions took possession of the popular mind, and these errors were unfortunately stimulated by the imperfect helminthological communications of some who undertook to instruct the people. In the month of January, of the following year, I published a few observations, the purport of which was to show not only that certain microscopic organisms, found in animals dying from cattle-plague, had little or nothing in common with true Entozoa, but that they were harmless "parasitic Protozoa," possessing more or less striking vegetable affinities. About a week previous other highly interesting researches on these so-called cattle-plague bodies were published by Professor Beale, but I did not meet with any person who had possessed at the time any adequate idea of the extent of the literature of the subject. In fact those who first saw these bodies thought they had stumbled upon organisms entirely new to science. I showed that similar or analogous organisms were to be met with in a great variety of animals, and likewise in the human body. They had been described under a variety of titles, such as worm-nodules, worm-nests, egg-

sacs, eggs of the common fluke, young "measles," corpuscles produced by muscular degeneration, psorospermiæ, stages of growth of gregarinæ, amœboid bodies, and so forth. In so far as the higher animals were concerned, Dujardin was the first to describe these structures in the mole.* This animal, however, having been fed upon earth-worms (whose perivisceral cavities were previously known to be constantly liable to harbour such parasites), there was no difficulty in accounting for the source of the psorosperms. In 1853, Hessling discovered psorospermial sacs in the muscular substance of the heart, not only of the ox, but also of the sheep and roe.† By him they were regarded as evidences of muscular degeneration. About ten years previously Miescher is said to have found similar bodies in the muscles of the mouse.‡ In 1857, Rainey described precisely similar structures taken from the flesh of swine; and, in his able memoir, he went so far as to maintain that these bodies were neither more nor less than the early stages of development of the common pork-measle.§ This view is still, I believe, maintained by Mr. Rainey; but, in support of his theory, I am not aware that he can reckon upon the aid of any helminthological writer of distinction. I may add, however, that for accuracy of description and detail, no communication either past or present is likely to surpass the one contributed by this able microscopist. In the year 1858, Gubler wrote a most important paper bearing on this subject, in which he related a case where twenty cysts existed in the human liver.‖ These sacs were of great size, mostly as large as a hen's egg, one of them being some six inches in diameter. Naturally enough, the largest was, during life, diagnosed to be that of an ordinary hydatid. Their nature, however, proved to be very different; for, on evacuating their contents (post mortem), they were found to harbour

* Hist. Nat. des Helm., 1845, p. 643.
† Sieb. and Köll. Zeitsch., 1853, bd. v., s. 196.
‡ Quoted by Leuckart and Siebold.
§ Phil. Trans., 1857, vol. 147.
‖ Mém. Soc. Biol., 1859, p. 657, and Gaz. Méd., 1858, tom. v., p. 61.

enormous quantities of minute corpuscles strictly analogous to those obtained from the ordinary psorospermial sacs. Gubler believed he had stumbled upon masses of eggs of *Distoma hepaticum*; but, to show how fallacious this notion was, it may be mentioned, that whilst the eggs of the common fluke measure as much as $\frac{1}{100}$ of an inch in diameter, the corpuscles in question were only some $\frac{1}{1500}$ of an inch long. Shortly after Gubler, similar bodies from the human liver were described by Virchow;* and in 1862, the subject was followed up by Dr. Dressler, of Prague.† Dressler, also in the human liver, found a number of pea-shaped bodies, the milky contents *(breisubstanz)* of which displayed a multitude of the characteristic corpuscular elements just referred to. These particles, already considered as equivalent to, if not identical with, the so-called pseudo-navicellæ of gregarinæ were now encountered by a variety of independent observers. From time to time Leuckart noticed these bodies in various animals, but with a caution natural to him he remarks:—"Concerning the nature of these formations I will not decide. To be candid, however, it appears to me to be in no way made out, whether the psorospermiæ are to be considered as the result of a special animal development, whether they, like pseudo-navicellæ, are the nuclei of gregariniform productions, or whether they are the final products of pathological metamorphosis."‡ Leuckart found these organisms in the intestines of a trichinised dog, also in a sheep and a pig severally fed with Trichinæ, in the muscles of another pig fed with psorosperms, and likewise, I believe, in the liver of various rabbits. He remarks that in swine these parasites, as compared with measles, are much more abundant. They were present in five of eighteen pigs, and also in two out of four sheep, whose flesh was specially examined by him in this relation. Valuable, however, as are the foregoing records, perhaps, in a pathological point of view, none equal in interest the observations of

* Arch. für Path. Anat., bd. xviii., s. 523.
† Quoted by Leuckart.
‡ Die Mensch. Paras., 1863, bd. i., s. 141.

Lindemann at Nischney-Novgorod.* This medical officer discovered psorospermial sacs attached to the hair of a girl who was being treated in hospital for chlorosis. I may remark, in passing, that the sacs in question bear a remarkable resemblance to the bodies which we now find in such abundance, not only in diseased but also in healthy cattle; and it would appear, from Lindemann's observations, that the affection is not very uncommon amongst the people of the locality just named. Curiously enough, in connexion with and attached to the same parasitically affected hairs, Lindemann noticed several moveable gregarinæ; and from this circumstance, in association with other considerations, he was led to believe in the existence of a genetic relation subsisting between the two kinds of bodies. He further expresses his conviction that the people contract the disease, if such it may be called, by washing themselves with water in which gregarinæ abound. Lindemann moreover refers to Lebert as having noticed similar parasites in a case of favus, and concludes that these organisms are of a vegetable nature. His opinion, though not shared by the majority of parasitologists, is nevertheless supported by the views of Robin,† Leydig,‡ and one or two others. Of still higher interest, also, are the observations of Lindemann respecting the occurrence of psorospermiæ in the capsule of the kidney of an hospital patient, who died with Bright's disease. The sacs in this case were remarkably small; nevertheless, their corpuscular contents unequivocally indicated their true nature. The pseudo-navicellæ measured only the $\frac{1}{5000}$ of an inch in diameter. Here, I might pause; yet, besides the above, many other able memoirs have appeared on the gregarinida, with which these bodies appear to be so intimately associated. Amongst the contributions, I would specially refer to those of Dufour;§ J. Müller;‖ Creplin;¶

* Quoted by Leuckart.

† Les Végét Paras., second ed., p. 291.

‡ Müller's Arch., 1851, p. 221, and Micros. Journ., 1853, p. 206, and Arch. für Anat. u. Phys., 1863, s. 191.

§ Ann. des Sci. Nat., 1837.

‖ Arch., 1841, s. 477.

¶ Müll. Arch., 1842, s. 61.

Kölliker;* Keferstein;† Stein;‡ Drummond;§ Lieberkühn;‖ and E. Ray Lankester.¶ To these I may add reference to my own previous observations.** I doubt if the interesting vegetable organisms described by Professor W. T. Gairdner†† can be referred to the group of parasites under consideration. At all events, from the foregoing remarks I think it will be seen that these spurious Entozoa, by whatever name called, were first discovered by Dufour in insects, by Müller in fishes, by Miescher in the mouse, by Dujardin in the mole, by Hessling in the larger quadrupeds, and by Gubler in man.

In this connexion, the results of my own oft-repeated examinations may be very briefly stated. In the flesh of cattle, I found psorospermial sacs varying from $\frac{1}{120}$ to $\frac{1}{12}$ of an inch in length, and in that of sheep from $\frac{1}{225}$ to $\frac{1}{80}$ of an inch. The bodies are enclosed in a well-defined transparent envelope, and even under low magnifying powers, their contents exhibit more or less distinct indications of segmentation. In some specimens the segments display themselves as a complete cell-formation, the contents of each individual cell being uniformly granular. Even under the quarter-inch objective, the contained granules are clearly visible, and on rupturing the sac, their peculiar characteristics are at once manifest. Each granule or corpuscle represents a pseudo-navicel, all of them displaying a tolerably uniform size, which I calculated to average about the $\frac{1}{2000}$ of an inch in diameter. Some of the corpuscles were round, others oval, several bluntly pointed at one end, many curved and fusiform, not a few being almost reniform. Under the quarter objective, highly refracting points or nucleoli were fairly visible in their anterior, but on employing the one-twelfth, I made out nothing more respecting the contents of the corpuscles.

* Zeitsch., 1848—49. † Gött. Anzeig., 1862, s. 1608.
‡ Müll. Arch., 1848, and Ann. Nat. Hist., 1850.
§ Edin. Physiol. Soc. Rep., June 19th, 1852.
‖ Müll. Arch., 1854, s. 349, and Reich. and Du Bois-Reym. Journ., Sept. 1865, and Micros. Journ., Jan. 1866. ¶ Ibid., 1866, p. 23.
** Proc. Linn. Soc., May 1862, p. 245, and Intell. Observ., 1862, p. 199.
†† Edin. Phys. Soc. Rep., for March 19th, 1853.

Turning to the more obviously practical aspect of the subject, I have again to remark, that these bodies have nothing whatever to do with the cattle plague. No one, it is true, who has had an opportunity of examining the flesh of any number of animals dying of rinderpest has, so far as I am aware, failed to discover them; yet in one or two isolated instances they appear to have escaped notice, and may possibly not have been present. When it is considered how long it takes us to examine a few grains weight of muscle carefully, it is obvious that the body of a large beast might contain many hundreds, or even thousands, of these spurious Entozoa, without our being able to detect their presence, except by a very prolonged and unnecessary investigation. In the few rinderpest beasts, portions of whose flesh I have submitted to the microscope, I should say that there were not more than one hundred of these bodies in an ounce of meat; but in the heart of a healthy sheep (which I afterwards ate) I calculated there were about a thousand parasites to the ounce, and in the heart of a healthy bullock (which likewise served me for a meal) their numbers were probably rather in excess of those in the sheep. Altogether, at two meals, I could not have swallowed less than 18,000 of these psorospermiæ; yet had they been Trichinæ, I should have hesitated to have dined off this food, however thoroughly well cooked the meat might have been. The fact is, that consumers of beef, mutton, and pork eat these bodies every day, but they take no harm because the parasites in question are not Entozoa in any proper sense of the term. I have known fine healthy beef to be returned by an alarmed customer to the butcher, when it was as good as any other meat in the market. I have examined various other meats such as veal, pork, and mutton, but in none have I found so great an abundance of these falsely so-called Entozoa as in beef which was perfectly healthy and sound. I should say, without exaggeration, that in one case a single ounce of the flesh contained upwards of 2000 parasites.

VII.

ON THE COMPARATIVE PREVALENCE OF DIFFERENT FORMS OF ENTOZOA INFESTING THE DOG, IN RELATION TO PUBLIC HEALTH.

NOTWITHSTANDING the rapid advances of Epidemiological Science in general, and of helminthology in particular, few persons are aware how intimately connected are the relations subsisting between man and the domestic animals in reference to entozootic diseases. Whilst our very existence is dependent upon a supply of certain animals, as sources of food and aids to civilization, it can nevertheless be shown that, under certain circumstances, any one of the most valued of our domestic quadrupeds may become an occasion of discomfort, disease, or even death. The truth of this general statement is sufficiently obvious in cases of ordinary accident, and also in those diseases (hydrophobia, for example) whose nature is either doubtful, or which, at the least, cannot be said to have a parasitic origin; but it comes out much more forcibly when we confine our attention exclusively to evils arising from Entozoa harboured by our domesticated animals in their capacity as "intermediary bearers." In these pages I have selected the dog as pointedly illustrating the correctness of the proposition just advanced; but, at the same time, I may add that, excepting, perhaps, the equine quadrupeds, I know of no important domestic animal which is not liable, by the agency of its parasites, to inflict on the human body one or other of the injuries just referred to—namely, discomfort, disease, or death.

I may also remark, in passing, that all our researches,

and more especially those of the experimental kind, tend to show how the evils just mentioned may be averted; and many of our investigations, having, in the first instance, a purely scientific object, have materially strengthened the results obtained by other researches having merely a practical aim. It may be said that the two methods should go hand in hand, or, at least, *pari passu*, in order to ensure brilliant results. Already helminthologists have obtained a considerable success; but it is just one of those successes in which the principal promoters are left without reward. Self-imposed tasks of this kind are more or less the prerogative of all the votaries of science, who, at least, have the satisfaction of knowing that they contribute to the public good.

To simplify and limit the subject-matter, I have given below a complete and revised list of all the Entozoa (species, varieties, and larvæ) at present known to infest the dog. In many respects, it is interesting to contrast this, as a whole, with the rather more extended list of human Entozoa recorded in this work (Part II.: Special Helminthology; p. 146); but, as will hereafter be seen, the mutual relations subsisting between certain members of the two series can only be established by a somewhat detailed notice respecting the individual forms. These are as follows:—

1. *Holostoma alatum*, Nitzsch.
2. *Spiroptera sanguinolenta*, Rudolphi.
3. *Dochmius trigonocephalus*, Dujardin.
4. *Trichosoma plica*, Rudolphi.
5. *Trichocephalus depressiusculus*, Rudolphi.
6. *Trichina spiralis*, Owen.
7. *Ascaris marginata*, Rudolphi.
8. *Eustrongylus gigas*, Diesing.
9. *Bothriocephalus latus*, Bremser.
10. *B. cordatus*, Leuckart.
11. *B. fuscus*, Krabbe.
    „ „ var. *reticulatus*, Krabbe.

B. fuscus, var. *dubius*, Krabbe.

12. *Tænia marginata*, Batsch.
13. *T. cœnurus*, Kuchenmeister.
14. *T. cucumerina*, Bloch.
15. *T. serrata*, Goeze.
16. *T. litterata*, Batsch.
17. *T. echinococcus*, Siebold.
18. *Pentastoma tænioides*, Rudolphi.
19. *Cysticercus* (*telæ*) *cellulosæ*, Rud.
20. *Filaria trispinulosa*, Diesing.
21. *F. sanguinis*, Cobbold.

} larval forms.

The above list, large as it is, might be very much extended if one chose to regard as true species a number of particular forms and varieties described by authors under other names, and severally considered by them to be well marked; but I could show, if it were necessary, that most, if not all, of the forms referred to are in point of fact identical with those here provisionally admitted as distinct. Probably the list is somewhat too extended as it is; yet I purposely refrain from entering at any length upon the exclusively zoological aspects of the question. In other words, I wish it to be understood that, as regards several of the forms here enumerated, I do not pledge myself to affirm that they are specifically distinct; nevertheless, taking them up serially in the order given, I have a few remarks to offer respecting each; and fortunately the end proposed will enable me to restrict my observations within the narrowest possible limits.

1. *Holostoma alatum*.—Speaking generally, flukes are sparingly found in the carnivorous mammalia; therefore that the Trematoda should be represented by a single species in the dog need not excite surprise. Though rare in the dog, this fluke is not uncommon in its congeners, the fox and wolf. Under a variety of names, its structure and relations have been carefully studied; but there is no ground for supposing that its existence is, either directly or indirectly, injurious to man.

2. *Spiroptera sanguinolenta.*—I am not aware that any one has distinctly indicated the presence of this parasite in dogs dying or destroyed in this country; but it appears to be tolerably frequent in France and Germany. I believe it to be identical with the round worm, which is known to be common in China, and not unfrequently to cause the sudden death of its canine host. Some years ago, a manuscript was placed in my hands, giving a detailed account of the ravages inflicted by these parasites; the paper, however, has been irretrievably lost. Professor Bennett, of Edinburgh, has in his possession a Chinese dog's heart which had burst, during life, from the pressure occasioned by a large number of these parasites lodged in the ventricles. One of the specimens presented to me by Dr. Bennett measures exactly ten inches in length. Dr. J. Lamprey has also found "Entozoa in the cavities of the hearts of dogs in China." In the year 1813 a round worm, probably referable to this species, was found in the heart of a dog at Paris; and there is some probability that the microscopic hæmatozoa, first discovered by Grube and Delafond in the blood of dogs, are genetically related to this species. Other remarks upon this point will be found further on.

3. *Dochmius trigonocephalus.*—This small parasite is probably not uncommon in this country; but it is either frequently overlooked, or disregarded on account of its apparent unimportance. It is not likely that the migrating progeny are the source of the verminiferous condition of the blood just alluded to. Its prevalence is certainly very variable in different countries or districts; but the only accurate observations bearing upon this point are those given by Dr. Krabbe in his recent Danish work.* Out of 500 dogs which he had examined (after death) at the Royal

* "Helminthologiske Undersögelser i Danmark og paa Island, med særligt Hensyn til Blæreormlidelserne paa Island." Af Dr. med. H. Krabbe. Kjœbenhavn, 1865. [The above facts are taken from the Danish work, for a copy of which I am indebted to the distinguished author. Soon after I had, with no small labour, translated part of this treatise, a French edition of this book appeared. Dr. Krabbe has likewise, through M. Blanchard, communicated a résumé of his able researches to the French Academy.]

Veterinary and Agricultural College at Copenhagen, only nine contained this parasite, whilst in 100 dogs dissected in Iceland it was entirely wanting. His experience, as regards its prevalence in other members of the canine race, coincides with that of observers generally; for he found this entozoon in three Danish foxes, and also in a blue or Arctic fox (*Canis lagopus*) which died in Dr. Kjærbolling's menagerie. It is worthy of remark that the last-named animal had originally come from Iceland. The importance of noticing these particulars, and of indicating all the rarer forms of canine Entozoa, will appear in the sequel.

4. *Trichosoma plica.*—This parasite, like the previous species, is much more common in the fox than in the dog; but the possibility of its occurrence in the latter should be borne in mind. So far as I am aware, the only direct proof we have of its liability to infest the dog rests upon the statements of Dr. Bellingham. The circumstance of its not inhabiting the alimentary canal will readily account for its not having been often seen, although it should afterwards turn out to be comparatively frequent in occurrence. The organization of the species has been sufficiently well investigated by Rayer; but its precise genetic relations remain to be cleared up. It is by no means improbable that the progeny of the preceding species, as well as of this entozoon, and of the form next to be noticed, passes through phases of development closely resembling those of *Trichina*. Thus it may happen that an inexperienced observer finding these larvæ, in the act of migrating, in the flesh of any "host," might be induced to conclude that he had discovered examples of the flesh-worm disease, when in truth he had only encountered the offspring of parasites totally distinct. Mistakes of this kind have occurred over and over again in reference to a minute and little-known parasite (*Olulanus*, Leuckt.) which infests the cat; and doubtless nearly all the sexually immature forms of parasites described as new species by their respective discoverers are the offspring only of some of their more or less familiarly known adult representatives.

5. *Trichocephalus depressiusculus.*—This parasite is likewise common to the dog and fox; and though, on the whole, more prevalent in the former, cannot be said to be frequent in either. I have only seen one or two examples. Out of 144 dogs dissected at Vienna, it was only noticed four times; and in sixty-two foxes examined at the same place, not one was encountered. On the other hand, Dujardin found this species present in two out of seven foxes dissected by him at Rennes. It does not appear to have come under Dr. Krabbe's observation in any of the 600 dogs which he examined in Denmark and Iceland. I regret that I retain no accurate data respecting the prevalence of this and several other of the less frequent parasitic forms which I have encountered from time to time in dogs. Until lately, the value of such peculiar statistical records, in reference to our domestic animals, did not fully strike me; but from 1855 to 1864 inclusive I noted down all the parasites encountered in less common "hosts." Part of the fragmentary results thus obtained are recorded in the "Proceedings of the Zoological Society" for 1861.

6. *Trichina spiralis.*—This interesting parasite is probably not indigenous, so to speak, in the dog; but the ease with which it may be transmitted, and the frequency of its introduction by experiment, oblige us now to class the "flesh-worm" with other canine Entozoa. Until lately, it was supposed that *Trichina* might be made to infest any warm-blooded animal; but the experiments conducted by Mr. Simonds and myself at the Royal Veterinary College in London, together with those on a more extended scale, by Drs. Pagenstecher and Fuchs, at the Zoological Institute at Heidelberg, incontestably prove that the so-called flesh-worm, as such, cannot be reared in birds. Having given in the foregoing pages a detailed account of these experiments, I will only again remark that I have repeatedly reared *Trichinæ* in the dog from the flesh of man and animals. Except in a very indirect manner, the dog itself is scarcely likely to communicate the flesh-worm disease to

man; nevertheless, if the flesh of a trichinised dog were eaten by us, the malady would be readily propagated. There would, moreover, be considerable danger in allowing trichinised dogs to roam at large; for the consumption of their flesh (after death) by other animals—such as rats, for example—would convey the disorder to new "hosts," which again might convey it to the pig, and ultimately to man. It has, indeed, been fully proved that, in some instances, swine have been infected by eating dead rats and the decomposing remains of other trichinised animals; and it is quite certain that rats, not previously made the subject of experiment, have been found to contain *Trichinæ* in their muscles. Dr. Krabbe informed me, by letter, that he has discovered trichinised rats in Copenhagen. The same thing has been noticed in Germany. Dogs and cats alike may become infected from this source. Bearing in mind, therefore, the variety of sources whence the dog may obtain *Trichina*, no one need in future be surprised at finding this animal affected with the disease; and, since the malady is so readily transmissible, care should be taken to destroy, thoroughly, all trichinised animal flesh, wherever and whenever encountered. In illustration of the facts above given, I may mention that I have caused *Trichinæ* to be transferred from the flesh of man to a hedgehog, then from the muscles of this hedgehog to a dog, and ultimately from the dog to a pig. It was in this last-named animal that I obtained about 16,000,000 *Trichinæ*—enough to have trichinised half the inhabitants of London, could each individual have been induced to partake of a minute portion of the uncooked flesh. As it was, Mr. Simonds and myself had some difficulty in persuading bystanders that the perfectly healthy-looking flesh had anything the matter with it; and one person actually carried off the heart of the animal as a perquisite. Fortunately *Trichinæ* do not stay in the substance of the heart, although they pass through it; and thus the person who ate part of that heart can by no possibility have sustained injury. Lastly, considering the abundant and satisfactory experiences now obtained by expe-

rimental helminthologists, I hold that there is no need for further experiments in this particular relation; from a hygienic point of view they are even undesirable.

7. *Ascaris marginata.*—Of all the parasites infesting the dog in England this species is the commonest. I have met with it in about thirty out of forty dogs dissected by myself and friends. It does not appear to be quite so common abroad: nevertheless, out of 144 dogs dissected at the Vienna Museum, it was present in no less than 104 instances. At Copenhagen, Dr. Krabbe found it present 122 times out of 500, or in twenty-four per cent. of the dogs he examined; but in Iceland, where other forms of canine Entozoa are extremely abundant, only two dogs out of 100 were found to entertain this species. Fortunately the common round worm of the dog is a very harmless parasite, so far as the health of the people is concerned, and it is apparently only in exceptional instances that it proves injurious to the dog itself.

8. *Eustrongylus gigas.*—This species is probably the rarest of canine Entozoa. The only specimen which I have seen as coming from the dog is the very perfect one preserved in the Museum of the Royal Veterinary College, London. Probably not one in 5000 dogs harbours this entozoon; but since it is liable to occur in man, and has several times been noticed in the congeners of the dog, the possibility of its becoming more frequent should be considered. The Museum of the Royal College of Surgeons, London, contains some fine examples and several dissections of this parasite. I have briefly described them in the "Catalogue of Entozoa" which the Council of the College entrusted me to draw up. As Professor Leuckart has recently referred to one of the valuable specimens (No. 19)* I may observe that the œsophagus is twisted in the particular dissection there displayed. That the "spiral" form is unnatural or accidental, so to speak, is obvious; but my description certainly implied otherwise, and must therefore be corrected.

* "Die Menschlichen Parasiten," band ii., s. 390.

9. *Bothriocephalus latus.*—Although there is some difficulty in determining the number of species of *Bothriocephalus* liable to infest the dog, it is generally agreed that this form is both human and canine, so to speak. In either case it is believed that the "host" acquires possession of the parasite by the consumption of fish. Dr. Knoch, of Petersburgh, believed he had succeeded in rearing this species in dogs by direct experiment with its embryos; but the necessity of an "intermediary bearer" has been clearly established by Leuckart. The presence of *Bothriocephali* in the dog in this country is very rare. One such cestode may be seen in the Museum of the Royal Veterinary College; and no doubt can be entertained that it belongs to this species.

10. *Bothriocephalus cordatus.*—This form is quite distinct from the above, and seems to be very abundant in the dogs of North Greenland, where it also occasionally infests the human body. In the dog it occurs in considerable numbers, and can scarcely fail to occasion the animal more or less distress; but we have no evidence to show that it gives rise to any inconvenience in the human subject, where, so far as is at present known, it either exist singly or in very small numbers. Its comparative abundance in the dog doubtless depends upon the more ready access which that animal has to the uncooked food containing the larvæ, which latter are supposed to abound in marine fish.

11. *Bothriocephalus fuscus.*—Dr. Krabbe, of Copenhagen, described a variety of pit-headed tapeworms obtained from dogs in Iceland, all of which appear to be distinct from the above species. He recognizes three separate kinds (*B. fuscus*, *B. reticulatus*, *B. dubius*), severally presenting marked features of their own; but he is not prepared to affirm that these characters have any specific value. I do not now discuss this point, but I may remark, in passing, that the preparations of *B. cordatus* sent me by Professor Leuckart, and the specimens *B. fuscus* presented to me by Dr. Krabbe, afford convincing proof of the distinctness of these two forms. Taking the *Bothriocephali* as a whole they only occur in

the ratio of five per cent. in Iceland, whilst Dr. Krabbe's investigations also show that in Denmark they are very much less frequent. In the 500 dogs examined at Copenhagen he only found one infested. It is by no means improbable that one or other of the forms will be discovered in this country.

12. *Tænia marginata.*—The tapeworms, properly so-called, are far more numerously represented than the *Bothriocephali;* and this is one of the commonest forms. The frequency with which I have encountered it leads me to conjecture its presence in at least twenty-five or thirty per cent. of our English dogs. In Denmark it is rather less abundant, being found, according to Krabbe, in fourteen per cent., whereas in Iceland it is remarkably prevalent, occurring in no less than seventy-five out of the hundred dogs which he examined. This parasite is seldom found alone, and, being the largest species liable to infest the dog, proves a formidable guest to its canine host. Up to the present time it has not been described as occurring in man; nevertheless I have seen portions of a tapeworm, apparently referable to this species, which I was assured had come from the human subject. From the first, I expressed the strongest doubts as to its source. The dog acquires this parasite by swallowing the large cestode larvæ which are frequently attached to the viscera of the sheep. Dr. Möller, as is well-known, tried to rear this parasite in himself, by swallowing the fresh uncooked larvæ (*Cysticercus tenuicollis*), but he did not succeed; and consequently we are not at present warranted in concluding that this species can under any circumstances develope itself in man. Mr. Simonds and myself succeeded in rearing several examples of this Entozoon in the dog by direct experiment. One or two instances are on record of the occurrence of the larvæ of this species in man; and I have elsewhere pointed to affirmative evidence in yet another case. In none of these examples is there any reason to suppose that the larvæ in question did any harm; yet it would be very unadvisable to allow of steps being taken which could promote its more frequent development within the

human host. Sewage-distribution where dogs abound would aid in securing this undesirable end.

13. *Tænia cœnurus*.—This species is likewise obtained from the sheep, though by no means exclusively so, as some imperfectly informed persons seem to suppose. Its prevalence in the dog, however, is comparatively rare. I have succeeded in rearing it without any difficulty, but I do not remember to have encountered the entozoon in any dog which had not previously been made the subject of a Cœnurus-worm-feeding. I need hardly say that its larval form (*Cœnurus cerebralis*) gives rise to the "gid" in flocks; but it also proves detrimental to several other animals. Indirectly, therefore, it affects man himself; yet, in this country, the "gid-disease" is seldom sufficiently prevalent to cause serious loss to the sheep-breeder. In other countries, Hungary for example, losses on this score are said to be very considerable. In Denmark Dr. Krabbe encountered this entozoon in only one per cent. of the dogs he dissected, but in Iceland he found it in no less than eighteen per cent. Clearly the "gid" must abound in that country. It is not generally known, even by those who have some acquaintance with helminthological matters, that the larvæ of this tapeworm sometimes infest the rabbit, giving rise to a formidable disease having its seat in the muscles and soft parts of that animal. At pages 121—123 of this work, I have already explained that Mr. C. B. Rose, F.G.S., was the first to direct attention to this important fact; but his explicit statements on this head have been either carelessly overlooked, or purposely disregarded. I regret again to observe that there are persons who, whilst fairly recording the information they obtain from foreign writers, appear to be studiously careful to avoid giving credit to the labours of their own countrymen. In the curious discovery before us we have another unexpected source indicated whence the dog may obtain the adult parasite, and thus, in its turn, afflict another group of creatures with the so-called bladder-worm disease.

14. *Tænia cucumerina*.—Without doubt this is the commonest

of all the forms of canine Entozoa. I have little hesitation in saying that it is present in two out of every three dogs living in this country, excluding, of course, puppies up to three months old. Krabbe found it present in forty-eight per cent. in Copenhagen, and in fifty-seven per cent. in Iceland. It appears to be very prevalent throughout Europe, and probably is more or less so in all other countries. So far as I know, the cucumerine tapeworm is a tolerably harmless parasite; but, notwithstanding the efforts of investigators, nothing certain is understood respecting its true larval source. In the adult state it sometimes occurs in prodigious numbers, and often associated with other parasites. In one valuable animal, a pointer, which died suddenly, I found between 500 and 600 of these parasites, associated with numerous examples of three other kinds of Entozoa. In this particular instance, it appeared clear to me that Entozoa, alone, were the cause of the dog's death. If the canine tapeworm species, *which are dangerous* to human life, became as abundant in this country as the harmless *Tænia cucumerina* has been shown to be, then our mortality would be increased by many thousands annually. This will, perhaps, be made more apparent in the sequel.

15. *Tænia serrata.*—Though by no means so abundant as the preceding, this parasite is nevertheless sufficiently common, whilst it is also more injurious to the bearer. Taking one variety of dog with another, I should say that it occurs in at least five per cent. of our English dogs; but in harriers and greyhounds I have little doubt that its presence is all but invariable. At Copenhagen Dr. Krabbe only encountered this parasite once; and in Iceland it seemed altogether wanting. The practice of giving the viscera of hares and rabbits to sporting dogs (which I have myself occasionally witnessed in the field) will always ensure the prevalence of this entozoon in our dogs. On five or six separate occasions I have reared this tapeworm in dogs by administration of the larvæ (*Cysticercus pisiformis*) taken from recently killed rabbits. It is one of the most easily reared of all the canine parasites; and, if due attention

be paid to the form and size of the head and its conspicuous crown of hooks, cannot well be confounded with its allies. The older helminthologists are not trustworthy respecting the prevalence or otherwise of this species on the continent, since they regarded several distinct forms as identical. Even Dujardin was sceptical respecting the distinctive characters, severally, of *Tænia serrata*, *T. marginata*, and *T. crassiceps* (of the fox).

16. *Tænia litterata*.—This well-marked form is described by Dr. Krabbe under the name of *T. canis lagopodis;* but, notwithstanding the priority of Rudolphi, I prefer the subsequent and more distinctive nomenclature of Batsch, especially also because the parasite is not by any means peculiar to the Arctic fox (*Canis lagopus*). Here we know nothing of this parasite; and it may be that it does not exist in England. On the other hand, its general resemblance to *Tænia cucumerina* may have caused it to be overlooked. It seems from Krabbe's extended researches to be altogether wanting in Denmark; but in Iceland he found it in twenty-one of the two hundred dogs which he there dissected. In the absence of any definite knowledge respecting its larval source, it may be assumed, as regards man, to rank amongst the so-called harmless species. The joints (judging from the specimens kindly sent me by Dr. Krabbe) have somewhat the appearance of those of Bothriocephali, owing to the central and ventral disposition of the reproductive organs. The orifices themselves, however, are not actually visible, though probably present in thoroughly mature segments.

17. *Tænia echinococcus*.—I come now to speak of a parasite of the highest interest in relation to public health. Though fortunately extremely rare in the adult condition, it is nevertheless sufficiently abundant to produce occasional fatal results by means of its larvæ. What is the actual amount of human mortality thus annually caused in this country it is not easy to say; nevertheless I know it to be something considerable; and there is reason to fear, in the absence of due precautions, that it may become much

greater. The larvæ or *hydatids* are familiar to every hospital surgeon. The adult parasite has frequently been reared by foreign experimenters; but although several hydatid-feedings have been administered to dogs in England, only one such feeding has, so far as I know, been attended with positive results. In the particular instance to which I refer, the facts were originally communicated to the Royal Society by Professor Nettleship. It is quite likely, however, that I had myself previously succeeded in rearing this parasite. I say "likely," because the subject of the experiment (a small black dog) having been liberated by an ill-designing person, I lost the necessary opportunity of destroying and examining my experimental animal. It is true that I had previously, in one or two other dogs, obtained only negative results; but in those instances none of the conditions likely to ensure success were so favourable as in the case of the liberated animal. At all events, Mr. Nettleship, following up these experiments, after a precisely similar method, obtained a complete success. The freedom of only ONE animal harbouring *Tœnia echinococcus* must be fraught with serious danger to the community; and yet it is to be feared that at the present time several dogs, thus infested, roam at large in this country. Certainly, I have no desire to add to their number. In Copenhagen, Dr. Krabbe encountered this parasite in *two* only out of 500 dogs; and yet the parasite is probably more abundant in Denmark than in England. In Iceland, on the other hand, where the mortality from the hydatid, or echinococcus-disease embraces one-sixth of all who die in that country, Dr. Krabbe found twenty-eight dogs out of a hundred harbouring this entozoon. Up to the present time no person has seen this parasite in any English dog which has not been previously made the subject of experiment.*

* Dr. Krabbe, in his recent work ("Helminthologiske Undersögelser," p. 58, or "Recherches Helminthologique," p. 60,) has commented on the sense of a sensational passage, which I have correctly translated from Leuckart in a former part of this work (p. 283). He thinks I have spoken somewhat unfairly of the doings of the "quacks" in Iceland. Most of them, he says, are homœopaths; and even "those who are not,

In order to obtain an approximatively correct notion as to the amount of echinococcus-disease prevalent amongst us, I devoted some three or four weeks in the winter of 1864 to an examination of the collections of Entozoa contained in nine of the principal Pathological Museums of the metropolis. The results of this search, independently of data derived from other sources of evidence, have convinced me that hydatids are far more prevalent than is generally imagined. In these collections I found no less than 195 instances of hydatid-disease out of a total of 368 cases of helminthiasis of all kinds. It is my deliberate belief that not less than 400 deaths annually occur in England from this source. Doubtless, if one could acquire correct statistical evidence respecting the amount of fatality from all the entozootic diseases combined, the rate of mortality from parasites would be considerably larger than this figure, by itself, implies; yet it fortunately happens that comparatively few of the other kinds of Entozoa at present prevalent in England lead to fatal results. Such cases are exceptional. It is equally certain, on the other hand, that much misery and discomfort is produced by the less hurtful species. A very great deal of good might accrue from the acquisition of more extended evidence respecting the prevalence or otherwise of all the known forms of Entozoa infesting man and the domestic animals in this country. A report of this kind, drawn up somewhat after the fashion of the present communication, would, I conceive, prove highly useful. Such a report should not be a mere record of helminthic epidemics collected from foreign sources, however valuable that may be, in itself; but we ought to have a sound and scientific contribution based upon a previous practical knowledge of the Entozoa, our information being extended by personal observations made under every variety of circumstance. It is one thing to give a literary *résumé* of the progress

treat their patients much in the same way as ordinary medical men." It simply comes to this, that, instead of *dog's excrement* forming with the aforesaid "quacks" a conspicuous or common remedy (as I had supposed from Leuckart's description), this nasty drug is now rarely administered, and by the grossly ignorant only.

of this department of science abroad, and another to record the actual position in which we stand in respect of entozootics at home. Except by exclusive and prolonged devotion to this subject no great ultimate good can be accomplished. A properly qualified person, always on the alert, could embrace many opportunities of special research which must of necessity be lost to the mere closet report-maker. In illustration of a lost opportunity, I may give an instance. In the "*Times*" of Saturday, July 14th, 1866, we read that "Among the claims presented to the Glamorganshire Court of Quarter Sessions, was a claim for poison used in killing stray dogs at Merthyr; another was from the chief inspector of the Swansea police for killing 100 dogs, at one shilling each; while the third was from a person who charged one guinea for the removal and interment of three cartloads of the dead bodies of the dogs." I have no doubt whatever as to the wisdom of destroying these animals; but I have to add that an examination of the bodies of these animals by a skilled helminthologist would have been particularly instructive and practically useful if conducted in relation to the great subject of Entozootics.

18. *Pentastoma tænioides.*—This parasite resides in the nasal cavity and frontal sinuses of the dog, but it is comparatively rare in England. In the young state, and under a different name (*P. denticulatum*), it is frequently seen in the human body on the Continent. Its mode of introduction into the latter "host" has not been ascertained with certainty; yet there can be very little doubt that the sneezing of an infested dog in the face of any person would readily transfer the eggs and embryos of the parasite from one "host" to the other. In like manner, and by the same means, the ova may be cast over and become attached to food, and then be subsequently conveyed to the human stomach. Fortunately its presence in man appears to be unattended with danger; yet any considerable number of these parasites could scarcely fail to produce more or less inconvenience. In Germany it seems to be rather abundant; for Frerichs (no mean authority) states that it

is "far more common in the human liver than the echinococcus." In confirmation of this statement, it has been shown to be present in from five to fifteen per cent. of *post mortem* examinations conducted in different German cities. I have myself frequently encountered this parasite in the juvenile state in animals, but not in the human body. The Pentastomes recently described by Dr. Aitken, from the human liver, belong to another and more formidable species. Strictly speaking, these creatures are not true Entozoa, although their habits often cause them to be classed as such. Their mode of introduction into the nostrils of the dog is readily accounted for, since the larvæ are constantly present in the flesh of herbivorous mammals, and must frequently, during the act of feeding, be brought in immediate contact with the dog's nose.

19. *Cysticercus* (*telæ*) *cellulosæ*.—Two or three authors (Gurlt, Chabert, and Hartwig) have stated that the common measle of the pork-tapeworm is liable to occur in the dog; and, since we know that it is occasionally found in man, there is no good reason for doubting the correctness of their conclusions. It has been found attached to the membranes of the brain, in the muscles, and in the cavity of the abdomen. If the dog were a thoroughly suitable "host," this larva would, in all likelihood, be much more common in the canine bearer than it is at present supposed to be. The possibility of its occurrence, and the probability of its having been frequently overlooked, should both be considered in reference to future investigations in this direction.

20. *Filaria trispinulosa*.—This little parasite, once found by Gescheidt in the eye of a dog, is probably only a sexually immature form of *Ascaris*. The specimen, however, was one-third of an inch in length, and recorded as a female. It does not appear to have since been met with.

21. *Filaria sanguinis*.—Under this title I have a few more words to say respecting the so-called hæmatozoa of MM. Grube and Delafond. These investigators, some years back, examined

480 dogs, and in nearly five per cent. they found Filariæ in the blood. The parasites were extremely minute, their diameter being less than that of the dog's blood-corpuscle! There can be no doubt that they were larvæ of some known, or unknown, species of nematode. I have already hinted that these hæmatozoa might be the brood of *Spiroptera sanguinolenta*; yet in only one instance did these authors find sexually mature nematodes (of comparatively large size) in the heart. In this case they obtained six specimens, "of which four were females and two males; the parasites being lodged in a large clot of blood occupying the right ventricle." They measured from one-half to three-fourths of an inch in length. MM. Grube and Delafond believed they had encountered a new species, and accordingly gave it the combined name of *Filaria papillosa hæmatica canis domestici*. In most of the dogs the entire circulatory system does not appear to have been examined; therefore it is quite possible that adult worms may have been present in more instances than the one specified—perhaps in several. If such had been proved to be the case, it might have been fair to have inferred a genetic relation between the microscopic hæmatozoa on the one hand and the worms in the heart on the other. As the matter now stands, we are in doubt as to the true adult representative of these minute *Filariæ*. With the verminiferous blood MM. Grube and Delafond performed a variety of curious experiments, but they did not, so far as I am aware, employ any worm-feedings. They satisfied themselves that the hæmatozoa could only live, as such, in the blood itself, and they estimated that the verminous dogs severally entertained from 11,000 to about 224,000 of these larvæ. In no case, however, did the infested animals appear to have suffered any inconvenience.

Conclusion.—I have thus, in a condensed form, brought together a large number of facts having reference to the frequency of occurrence and to the great variety of Entozoa liable to infest the dog; this record embracing all the more essential details I have previously made public. The way in which I have treated

the subject is somewhat novel, my object being to open up a new field of inquiry, bearing more or less closely on questions of public health. The full importance of helminthology in relation to entozootics can only be understood by expositions of this kind, based upon investigations extending over a long period of time. To do justice to the subject, one must not only be acquainted with the commoner forms of Entozoa, but also, to some extent, with the rarer. The experimental method enables us to determine the origin and course of development of many forms, and helps us to discriminate between the harmless and baneful species. Systematic zoology, apart from its own abstractedly scientific value, is of great assistance in aiding our arrangement of the facts in a methodical and easily understood manner. A consideration of all the known data relating to any one particular entozoon often permits us to state precisely to what extent the species is injurious to the human race, and whether or no it is a hurtful parasite during its larval stages of development in the particular intermediary bearer which it happens to infest. We are also frequently in a position to point out, on the one hand, what circumstances are sure to increase, or, on the other, to decrease, the prevalence of any particular species. We can even go further than that, and show how certain parasitic forms may be entirely eradicated. At all events, we have it in our power, both to diminish the number of human sufferers from entozootics, and to check, if not entirely to prevent, the invasion of these endemics. Substantial results of this kind being patent to all intelligent people, we can afford to disregard the policy of the ignorant who deride our labours.

VIII.

ON THE ENTOZOA OF GAME BIRDS AND ON THOSE OF THE COMMON FOWL.

THE year 1867 will long by remembered by sportsmen, on account of the scarcity of grouse caused by an epidemic exclusively effecting these birds. Not that there was any essential difference between the phenomena of the disorder then presented, as compared with the features of the disease recorded from previous experiences of this sort; but, the epidemic being somewhat more virulent than usual gave rise to the notion that we had here to deal with a new malady. As some people were strongly impressed with the belief that the epidemic was due to the prevalence of Entozoa in grouse, I undertook to draw up a brief statement showing the present state of our knowledge respecting the internal parasites of game birds generally and of the grouse in particular. My observations on this head were originally communicated to the meeting of the British Association, held at Dundee in the above-mentioned year; but, beyond certain imperfect notices which appeared in the public journals at the time, no particular record of them has been published. In the persuasion that the following summary of facts may serve as a useful point of departure for future inquiries, I do not hesitate to give them a more formal expression.

As I have just said, the idea of the "grouse-disease" being due to the prevalence of cestoid Entozoa was manifestly erroneous. Not only were the symptoms exhibited by the infested birds entirely at variance with such a notion, but the *post mortem* appearances

distinctly proved that the disorder was due to other causes. The mere fact that in several or even in many instances tapeworms were found in the intestines of the birds afforded no positive proof of injurious influence from this source, especially since there was no evidence showing that the parasites were more abundant than usual. It is not easy for people unacquainted with the phenomena of parasitism, and with the mode of distribution of the Entozoa, to shake off the very prevalent notion that tapeworms are generated only in diseased or unhealthy animals. This old and erroneous idea has been handed down from age to age, and it will probably prevail amongst us for many years to come. It is not my present purpose to offer any opinion respecting the actual cause of the grouse-disease; but I am prepared, nevertheless, to show that it has no necessary connection either with the presence or absence of Entozoa. It is one thing to admit the occasional destruction of game birds from the prevalence of tapeworms, and quite another to suppose that their presence could give rise to the singular morbid appearances which were found in the diseased grouse of the year 1867. Amongst all the various birds and mammals from which I have removed Entozoa, *post mortem*, I never remember to have found the alimentary canal inflamed, and certainly there was no such disorganisation of the liver and lungs as obtained in the case of birds dying of the "grouse disorder." No amount of tapeworms could ever give rise to grangrene and pyæmia of these organs; and even in cases where the parasites are sufficiently numerous to prove fatal to the "bearer," death never supervenes in the manner shown to have taken place in the grouse affection. As a rule, tapeworms and other adult forms of Entozoa in animals are liable to produce emaciation, gradually destroying life when present in any considerable number; but in the "new disease" the birds were frequently quite plump at the time of their death, and the numbers of Entozoa present were never very remarkable. In the striking instance (recorded by Mr. J. K. Lord in *Land and Water*) in which the lungs, liver, and heart of a

grouse, shot on the wing, were excessively diseased, no tape-worms existed; and, in short, without going into further details, I dismiss the notion of any entozootic influence as producing that remarkable epidemic. The subject of Entozoa in birds, apart from this epidemiological question, is not without interest in itself; and, although it cannot be said that our knowledge of avian Entozoa is on a par with that we possess of the mammalian parasites, yet a mere glance at the following list will show that we are not entirely ignorant of the many varieties liable to infest our game birds. Their abundance in the common fowl will doubtless surprise most persons; but they may take comfort in the assurance I can offer as to the perfect harmlessness, so far as man is concerned, of any of the parasitic species. In other words, it may be safely affirmed that the common fowl and all kinds of game, however imperfectly cooked, may be eaten by man with entire impunity:—

List of Entozoa infesting game birds and the common fowl.	Pheasant.	Capercaillie.	Black Grouse.	Red Grouse.	Grey Partridge.	Red Partridge.	Quail.	Fowl.
Monostoma verrucosum, Zeder								*
Distoma oxycephalum, Rudolphi								*
Distoma ovatum, Rudolphi								*
Distoma lineare, Zeder								*
Distoma dilatatum, Miram.								*
Distoma fuscatum, Rudolphi							*	
Ascaris vesicularis, Froelich	*	*		*	*		*	*
Ascaris gibbosa, Rudolphi								*
Ascaris inflexa, Rudolphi		*						*
Ascaris compar, Schrank			*	*	*		*	
Spiroptera hamulosa, Diesing								*
Spiroptera helicina, Molin						*		
Dispharagus nasutus, Dujardin								*
Dispharagus spiralis, Molin								*
Sclerostoma syngamus, Diesing	*				*			*
Trichosoma longicolle, Rudolphi	*	*	*		*			
Tænia malleus, Goeze								*
Tænia microps, Diesing		*	*					
Tænia calva, Baird				*				
Tænia linea, Goeze				*	*		*	
Tænia infundibuliformis, Goeze	*							*

This list comprises twenty-one forms; but I am by no means prepared to say that they are all really distinct species. In one or two cases the so-called species are probably only varieties. Provisionally regarding the list as correct, we have here to deal with three separate groups of Entozoa—namely, flukes, round worms, and tapeworms. The series of flukes consists of six species, five of which are recorded as infesting the fowl only, the sixth being confined to the quail. I may mention that the ovate fluke (*Distoma ovatum*), though not apparently prevalent in game, is common to many other birds than the fowl. It has once or twice been found in the eggs of the fowl. The round-worm series comprises ten species. Of these, seven infest the fowl. The most important is that form (*Sclerostoma*) which gives rise to the "gapes." As I have already written on this subject at considerable length in the present work (pp. 84—91) I will only add that no new facts of any importance have come to light since the time of publication of my previous papers on this subject in the "Proceedings of the Linnean Society" and in the *Field* for June 22, 1861. The round worm of the cæcum (*Ascaris vesicularis*) probably infests all game birds; for, in addition to the habitations here recorded in the fowl, pheasant, capercaillie, grouse, partridge, and quail, I have found it infesting the cheer-pheasant, the ring-necked pheasant, and the black-backed kaleege (*Euplocoma*). The curved round worm (*Ascaris inflexa*), like the ovate fluke, has been discovered within the white of the egg of the common fowl. Many birds are liable to have a species of nematode (*Spiroptera helicina*) take up its residence in the foot, causing tumours in the neighbourhood of the joints; but, so far as I am aware, the parasite has not been observed in the game birds of England, Scotland, or Ireland. With the above exceptions, it does not appear that any of the remaining forms of round-worm are capable of giving rise to severe disease in these birds. As regards tapeworms the case is somewhat different; yet, even here, the health of the avian "bearer" only suffers when, as occasionally happens, the parasites

exist in prodigious numbers. I have been repeatedly asked to give the scientific name of the particular species which infests the grouse. In the foregoing list it is entered as the *Tænia linea*. I have little doubt that this is the species, although no one in this country has hitherto recognized it as such. Specimens of grouse tapeworm, from the collection of the late Dr. Johnson, of Berwick, have been described in the "Catalogue of Entozoa contained in the British Museum" as referable to a distinct species (*Tænia calva*). In the list at the previous page I have recorded this latter form as distinct; but I am of opinion, judging from Dr. Baird's description, that it is identical with the variety long ago known to infest the partridge and the quail. To show how prevalent this parasite is in the latter bird, I may mention that Rudolphi found it in six quails out of seven. In the fully developed state, its length varies from four to twelve inches. In specimens examined in the year 1867, I found the diameter of the head only $\frac{1}{80}$th of an inch, that of the neck $\frac{1}{250}$th, that of the proboscis (and its circle of hooks) $\frac{1}{180}$th, that of the primitive egg-capsules $\frac{1}{200}$th, whilst that of the true egg itself was only about the $\frac{1}{1000}$th of an inch. All authors have described the proboscis as unarmed; but I demonstrated the existence of hooks which, estimated roughly (for I did not succeed in isolating them), appeared to be scarcely the $\frac{1}{1400}$th of an inch in their longest diameter. Nothing is actually known respecting the cysticercal stages of development of this tapeworm; but it is quite clear that the embryos are remarkably minute, and thus well fitted to take up their temporary residence within the bodies of small insects or their larvæ. It is highly probable that these are the sources whence the larval cestodes are obtained by the birds in whose intestines they are subsequently found in their final stages of growth. The subject of bird-tapeworms is one of very great extent; and, with the exception of Dr. Krabbe, of Copenhagen, I know of no one specially engaged in working at this interesting group. From time to time I have encountered a variety of cestoid Entozoa in birds, but hitherto I have been able to do

little more than record their occurrence. So far as my obse tions have extended, the forms are exceedingly numerous; the species cannot be satisfactorily defined or identified witl the aid of a prolonged and careful microscopic search. The offered in these pages is, doubtless, imperfect; yet, as it is first of the kind put together in this particular form, I am clined to believe that it will be found useful, if only as a tabl reference.

IX.

OBSERVATIONS ON THE DISTOMA CLAVATUM OF THE SWORD-FISH AND ON THE SO-CALLED DISTOMA ELEPHANTIS OF THE INDIAN ELEPHANT.

THE following remarks comprise the substance of two papers which I separately communicated, one to the Linnean Society, and the other to the Meeting of the British Association, held at Norwich in 1868. The brief account of the elephant's fluke has since appeared in the "Microscopical Journal," jointly edited by Dr. Lankester, F.R.S., and his son, Mr. E. Ray Lankester.

During my stay at Lynn, Norfolk, in August, 1865, a fine example of the common sword-fish *(Xiphias gladius)* was cast ashore in the estuary. Although quite dead when discovered, the creature was in a tolerably fresh condition; and when subsequently dissected by Dr. John Lowe (who has for years past devoted considerable attention to the zoology of the Norfolk estuary) and myself, some of its internal parasites were still alive. Respecting this interesting fish, I will only add, by way of proof as to its full growth, that it measured exactly ten feet and two inches from the tip of the snout to the end of the upper division of the tail.

Five different species of Entozoa were encountered in the flesh and intestinal canal of this "bearer"—namely, *Distoma clavatum*, *Ascaris incurva*, *Bothriocephalus plicatus*, *Tetrarhynchus attenuatus*, and a form of *Scolex* referable to a second species of the last-named genus. On the first-named of these parasites I offer the following observations,—not, indeed, with the view of seeking

to establish the existence of several new species by splitting up an old one, but rather for the purpose of showing that the *Distoma clavatum* may be viewed as representing a variety of forms hitherto regarded as separate species by helminthological writers.

Distoma clavatum.—Five examples of the trematode, which I believe to be referable to this species, were found in the stomach of the fish. They severally varied in length from four lines to two inches; being dead, and apparently only very slightly, if at all, decomposed. They differed somewhat in shape; but all had the so-called head and neck directed backwards. In one example the anterior slender moiety formed a right angle with the body proper, the margin of the ventral acetabulum, viewed from before, being, as it were, placed on a level with the oral sucker. Below the ventral sucker, the two largest specimens were distended with eggs and black pigmentary matter, all of them showing, internally, a dark spot near the centre of the neck. All of them likewise exhibited more or less well-marked transverse rugæ, extending from the root of the ventral sucker to the lower end of the body. The last ring thus formed surrounded a distinct caudal orifice, representing the outlet of a largely developed contractile vesicle. The eggs presented an average longitudinal diameter of $\frac{1}{100}''$. Some other points bearing upon the question of specific difference will be incidentally mentioned below.

When occupied, a few years since, in revising the collection of Entozoa contained in the Museum of the Royal College of Surgeons, London, I encountered a variety of parasites without any labels attached, or any mark capable of guiding one as to the source of the specimens. Amongst these were several flukes, which, though differing from each other in respect of size and shape, appeared to be identical species. One of these specimens I afterwards found to be the individual example of *Distoma clavatum* long ago described and figured by Professor Owen in the Zoological Society's Transactions. Several of the others I have since (by comparing them

with specimens deposited in the British Museum) clearly made out to be part of a series contributed by Mr. George Bennett; but the College Museum-stores contained yet a third group of specimens, whose history had hitherto evaded all my endeavours to unravel it. The large fluke described by Professor Owen was formerly in the collection of the Rev. Lansdowne Guilding; but we do not know from what fish it was obtained. In Dr. Baird's catalogue, the specimens presented by Mr. Bennett are stated to have come from the stomach of a Bonito; and it is not improbable that Mr. Guilding's specimens, as well as many others whose history is wanting, may be referred to the same "host." Be that as it may, the specimens in question not only differ very markedly among themselves, but also, in some respects, from many other forms referable to the same species. I here allude to the various specimens described by systematic helminthologists, some under one title and some under another. In fact, a species-splitter can point to five or six tolerably distinct forms, which, in my view, ought to be regarded as specifically identical. To prove this, however, it is necessary to investigate the matter with some care, and to pass in review all the more important notices which have from time to time appeared.

In the year 1730, M. Garsin, a surgeon in the employ of the Dutch East-Indian Company, and Corresponding Member of the French Academy of Sciences, first described this worm, under the generic title of *Hirudinella*. He says:—"Cet insecte tiré de l'estomac de la Bonite ne vécut qu'environ deux heures. Exposé à l'air il étoit languissant, et reprenoit de la vivacité dans de l'eau de mer. Il diminua sensiblement de volume pendant qu'il vivoit encore."* M. Garsin's brief description is accompanied by three figures, two of them giving a plan of the possible movements of the head and neck on the one hand, and of the body on the other, the ventral sucker being the fixed point. His specimens do not appear to have exceeded one inch and a half in length.

* Histoire de l'Acad. des Sciences à Paris, 1730, p. 44.

In 1774, Pallas described a trematode under the generic and specific name of *Fasciola ventricosa*. It measured two inches in length; but we are left in doubt as to whether it was actually obtained from a fish. All that he says regarding its source is as follows:—"Ex Amboyna missum fuit singulare hoc molluscum, quod ad aliud quam Fasciolarum genus referre non potui, in quo quasi gigas erit."* He remarks upon its pale white colour, and notices particularly the soft elastic body proper, which when wounded gave out a dark matter resembling soot. This material, when examined with the microscope, appeared perfectly fresh; it was certainly not the result of decomposition. Pallas also gives many other details respecting the structure of the parasite, accompanied by a figure.

In 1790, Menzies likewise described and figured a fluke about two inches long, which, though differing remarkably from the foregoing in respect of shape, is nevertheless identical. His account of the parasite is recorded in the first volume of the Linnean Society's Transactions, and he calls it *Fasciola clavata*:—"It is of whitish colour, somewhat pellucid, discharging at its mouth a black-coloured fluid which can easily be perceived through its body. I have often found it," he adds, "in the maws of the Bonito, between the tropics in the Pacific Ocean."† Notwithstanding the similarity of description, Menzies does not appear to have recognized the identity of his worm with that described by Pallas. Professor Owen, however, subsequently established this identity, and referred to this species as the *Fasciola clavata* seu *ventricosa*.‡ On the other hand, the British Museum Catalogue represents Pallas's worm as specifically distinct from that of Menzies, but as identical with the specimen described by Professor Owen from Mr. Guilding's collection.

In 1802, Bosc described and figured a trematode under the

* Spicilegia Zoologica, Fascic. x. (1774), p. 18.
† Trans. Linn. Soc., vol. i. (1790), pp. 187, 188.
‡ Trans. Zool. Soc., vol. i. (1835), p. 382.

title of *Fasciola fusca.* This he obtained from the intestines of a Dorado. In form it differs considerably from any of the foregoing species, with all of which, however, it is probably identical. Bosc's description runs as follows:—"Brune, la partie postérieure très-renflée, presque ovale; la partie antérieure mince, cylindrique, inégale, avec deux petits tentacules en dessous. Le suçoir de l'anus très grand."* Bosc recognized the identity of his worm with the *Distoma Coryphenæ* of Rudolphi; and systematists generally have adopted his synonymy. In the British Museum Catalogue, the *Fasciola fusca* and *F. ventricosa* of Pallas are regarded as one and the same species. If two small appendages did really exist below the oral sucker, then Bosc's worm is certainly a distinct species. I have never seen anything resembling this amongst the trematode parasites—though the exserted penis might very well be mistaken for one such process. Helmintholo-gists, generally, appear to have doubted the existence of such developments.

In 1827, Nardo obtained two very large flukes from the stomach of a fish captured in the Gulf of Venice during the month of September. He calls the fish *Prostostegus prototypus*, which appears to be the same as the *Luvarus imperialis* of Rafinesque. One of the parasites being no less than five inches in length, and nearly half an inch in breadth, he appropriately named the species *Distoma gigas*, believing, naturally enough, that he had to deal with a new species. His description is as follows:—"Distoma teres, rubrum, retractile; poro ventrali minimo cujus apertura magna, rotunda, ciliata; poro antico terminali, parvo; collo brevi, retrorsum divergente, extensili, apice angusto, basi lato; cauda longa, postice incrassata et in apice obtuso osculo donata."† Here, again, a character is introduced, the nature of which it is extremely difficult to understand. I allude to the alleged ciliated condition of the ventral sucker, an appearance perhaps due to a

* Hist. Nat. des Vers, vol. i. (1802), p. 271.

† Isis, for 1833, p. 523; from Heisinger's "Zeitschrift," 1827, p. 68.

wrinkled state of the lip. Apart from this, I see no reason for supposing this parasite to be distinct from the *Distoma clavatum* procured by Mr. Guilding, or the *Fasciola ventricosa* described by Pallas. The intestines of the fish in question harboured another trematode parasite, to which Nardo applied the title *D. Rayneri-anum*. This appears to be a distinct species; but its size is not stated. Unfortunately, Nardo gives no figure of his *Distoma gigas*. It is the longest fluke at present known.

In the year 1835, Professor Owen communicated to the Zoological Society the anatomical memoir to which I have already made reference. In his paper he ably discusses several questions relating to the structure of *Distoma clavatum*, and throws considerable light upon the organization of this species. He quotes the previous writings of Pallas, Rudolphi, and Menzies, and establishes the identity of *Fasciola ventricosa* and *F. clavata*. Although some particulars are wanting respecting the precise mode of termination of the digestive tubes, I think that there can be no doubt as to the propriety of retaining this species amongst the true Distomes. I believe that the large "lateral cavities" described by Professor Owen are neither more nor less than the somewhat unusually distended alimentary cæca. In this particular, every helminthologist is familiar with the varieties presented by different species of Trematoda. At all events, there is here no good ground for retaining the generic name *Fasciola;* and still less are we called upon to recognize any of the forms under the title of *Hirudinella*, although Garsin first described the species under this generic title.

In 1845, Dujardin placed the worm with the true Distomes, yet, at the same time expressed grave doubts as to whether it were, in any sense, a fluke. "Ce ver," he remarks, "n'est certainement pas un distome ni même un trématode. Si sa forme extérieure et ses deux oscules lui donnent quelque ressemblance avec les distomes, sa structure musculeuse le rapproche davantage des Gordius, et son tégument ressemble à celui des siponcles."

M. Dujardin carefully examined the specimens preserved in the Paris Museum; and, with regard to one particular example, described as "Fasciola, trouvé dans la mer de Nice," he says, it presents "une certaine analogie avec le prétendu *Distoma clavatum*." Manifestly Dujardin himself was somewhat puzzled by the resemblances in question. He does not appear to have had any opportunity of examining fresh specimens; yet he mentions the species as tolerably common in the Bonito, being also occasionally present in the Tunny. At all events, it would appear, by evidence derived from various sources, that the rightly so-called *Distoma clavatum* is not unfrequently taken from the ocean quite independent of its piscine "bearers."

In reviewing the foregoing notices, it is, of course, open to any naturalist to doubt if these parasites, one and all, can be said to refer to the same species; yet, notwithstanding the many divergences of statement, if any one will take the trouble to examine all the specimens preserved in this country I think he will arrive at the conviction which I expressed at the outset of my remarks. It is a comparatively easy task to name afresh every entozoon which happens to come into one's possession; but to ascertain how often it has previously been described may involve a good deal of labour. As an illustration of the truth of this observation, I subjoin a list of the synonyms which I believe to be referable to the species under consideration:—*Distoma clavatum*, Rudolphi =*D. Coryphænæ*, Rud.=*D. gigas*, Nardo=*Fasciola clavata*, Menzies=*F. Coryphænæ*, Bosc=*F. Coryph. Hippuridis* and *F. Scombri Pelamidis*, Tilesius=*F. fusca*, Bosc=*Hirudinella marina*, Garsin =*H. clavata*, Baird.

Probably we may here also include Rudolphi's *Distoma tornatum;* but I have never seen the caudal extremity of *D. clavatum* projected to the extent described by Dujardin as occurring in *D. tornatum* though I think it quite capable of becoming so. Diesing, in my view, gave this accidental invaginating process too much prominence as a specific character when he wrote in regard to

D. tornatum, "Cauda longissima, gracilescente, moniliformi," overlooking the circumstance that the tail is normally truncated posteriorly. Whether the correctness of my opinions respecting the synonymy of *D. clavatum* be admitted or not, I am confident, as regards certain other reputedly distinct forms of this genus and its allies, that they have had a common origin.

Distoma Elephantis.—On the 24th of June, 1868, I received a small bottle containing two flukes. It was accompanied by a note from Dr. Baird, F.R.S., stating that the specimens had been transmitted to him from India by Dr. Hugh Cleghorn. On the phial itself was a brief notice to the following effect:—"Distoma taken from liver of elephant at Rangoon, forwarded for classification to Professor Cobbold by Veterinary Surgeon J. Thacker, Madras Army." This is all I know of the history of these two Entozoa. On naked-eye inspection it was clear to me that they were identical in character with a larger series of specimens exhibited by Professor Huxley during the delivery of a course of lectures at the Royal College of Surgeons in the spring of the same year. Having communicated to Prof. Huxley the facts as above stated, I received permission to make use of his set of specimens, originally fifteen in number, for the purposes of comparison and description. This series was placed in the hands of Mr. Flower, F.R.S., last February, and, under his direction, six of the individuals were subsequently selected and mounted, with the view to their being added to the valuable collection of Entozoa contained in the Hunterian Museum.

The first thing to be determined was as to whether or not these flukes were new to science. In this relation, therefore, I have to remark that so far back as the year 1847, Dr. Jackson, in his "Descriptive Catalogue of the Anatomical Museum of the Boston Society for Medical Improvement," incidentally mentions the occurrence of flukes in the Indian elephant. Though several examples were removed from the biliary ducts and duodenum, along with many specimens of *Ascaris lonchoptera*, it does not

appear that any of them have ever been properly described. In all likelihood (if, happily, the flukes are still preserved in the Boston Museum) it will be found that they specifically correspond with those now in our possession. I may add that the late C. M. Diesing, in the appendix to his well-known "Systema Helminthum," had already, in 1850, noticed Jackson's statement (vol. ii., p. 560), and also subsequently in his more recent "Revision der Myzelminthen" (s. 50). In the last-named work, which bears the date of 1858, Diesing still regarded the entozoon as a doubtfully distinct form, allowing it, however, to appear under the title of *Distomum elephantis*. In my synopsis of the Distomidæ, communicated to

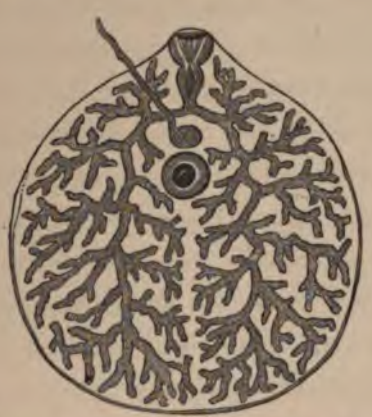

FIG. 3.—Fluke (*Fasciola Jacksoni*) from the Indian Elephant, showing the arrangement of the digestive canals, the suckers, reproductive vesicle, and incompletely protruded intromittent organ. (× 6 diam.)—Original.

the Linnean Society in 1859, I admitted it, conjecturally, as a good species ("Proceed." for 1860, Zool. Div., vol. v., p. 9). That determination has since proved correct, for it now turns out that this elephant-fluke (judging from those that were placed in my hands) is not only a distinct species, but that it differs also generically from the Distomes properly so-called. In point of fact, it is a sort of transition-type between the genera Fasciola and Campula (Fig. 3). This last-named genus I established in 1857 for a form which I found in the liver-ducts of the common porpoise; but as the species under consideration comes rather more closely to

the genus Fasciola than to Campula, I shall not seek to complicate matters by adding another generic type. Anyhow, the nomenclature must be altered; consequently I herewith correctly name and describe the species as follows:—

Fasciola Jacksoni, Cobbold.—Body armed throughout with minute spines, orbicular, usually folded at either end towards the ventral aspect, thus presenting a concavo-convex form; oral sucker terminal, with reproductive papilla about midway between it and the ventral acetabulum; intromittent organ $\frac{1}{4}''$ in length; digestive apparatus with two main zigzag-shaped canals, giving off alternating branches at the angles thus formed, the ultimate cæcal ramifications together occupying the whole extent of the body; length, when unrolled, from $\frac{1}{3}''$ to $\frac{5}{6}''$; breadth, $\frac{1}{3}''$ to $\frac{1}{4}''$.

In all the specimens examined by me the alimentary tubes were occupied with inspissated bile; so completely so, as to supersede the necessity of any attempts at artificial injection. I ought also to add that the cæca and branches of the alimentary system are more numerous than actually represented in the accompanying illustration. Mounted examples of this entozoon may now be seen in the Museum of the Royal College of Surgeons. They have been added to the Hunterian collection since the publication of the catalogue of Entozoa. Nos. 8 & 9 in that list are examples of the above described *Distoma clavatum*, "probably obtained from a Bonito."

X.

ON THE QUESTION OF ORGANIC INDIVIDUALITY, ENTOZOOLOGICALLY CONSIDERED.

THE following views were originally expressed in very similar terms in a paper which I communicated to the Linnean Society:—

When Professors Carpenter and Huxley promulgated their original and philosophic views respecting the question of animal individuality, they virtually established a general proposition regarding the constitution of the "zoological individual," which forms an admirable stand-point by whose aid I think it possible to interpret the significance and relations of that complicated series of life-phenomena in the Entozoa, hitherto so much misunderstood and undervalued.

The general proposition here referred to was formally embodied in the announcement that the "zoological individual" comprises the sum-total of the phenomena displayed by all the products of a single ovum, or, to employ Professor Huxley's own words, "the *individual* animal is the sum of the phenomena presented by a single life."

Many physiologists have asserted that the human frame, during its life-period, is represented by several epochs, each of which is, for the most part, distinctive and separable in so far as actual matter or tissue is concerned, but inseparable and almost indistinctive as regards mere appearances, whether external or internal. In other words, during man's growth we have a definite succession of life-phases; these being, in my view, analogous to, if not in

any sense homologically identical with, the distinctive and peculiar temporary forms of life so notably characteristic of certain of the lower animal types, particularly of the Entozoa.

Taking, as it were, a bird's-eye view of the whole zoological series, these temporary life-phases display every degree of distinctiveness from the almost imperceptible up to the separable, free, individual-like form for the designation of which Professor Huxley has felicitously proposed the term "zoöid." All these phases are now known to be phenomena of growth, metamorphosis, and gemmation, there being no such thing as "alternate generation" in the more legitimate sense of this phrase.*

Applying these principles to the interpretation of the phenomena of entozootic life, some very curious results appear to be attainable when we come to deal with the more complicated forms. Starting, however, with a species where the individual is represented by simple, non-metamorphosed life-phases, we necessarily encounter the almost indistinctive conditions of ordinary growth. Thus I select, in the first instance the *Trichina spiralis* whose life-phenomena (according to the synoptical method initiated in this work, at p. 115) may be tabulated as follows:—

Zoological individual (*Trichina spiralis*).
- *a.* Ovum in all stages.
- *b.* Intra-uterine embryo.
- *c.* Free embryo or migrating larva.
- *d.* Resting or sexually immature larva (finally encysted).
- *e.* Free, sexually mature, intestinal *Trichina*.

Now, although the various larval stages above indicated bear a general resemblance to the adult *Trichina*, we have, even here, some faint traces of "epochs" which, were they only rather more strongly pronounced, would enable us to draw lines of demarcation. In some instances the life-epoch may be homologically identical with a temporary bud, but it may also comprise a multitude of

* Probably, in the case of Rhabditis we have a true alternation of generation, or, to say the least, a true sexual dimorphism in animals (see Leuckart in "Nachrichten von der Königl. Gesellsch. der Wissensch. zu Göttingen," No. 8, April 19, 1865, s. 227).

gemmæ. Each such successive life-epoch, whether distinctive or indistinctive, separable or inseparable, I propose to call a *biotome;* and when two or more such life-divisions are recognizable, I propose to call them "secondary" or "tertiary" biotomes, as the case may be. I would observe that the term "biotome" is not designed to supersede the term "zoöid," but rather to limit the latter to an individualised, free, constituent portion of the "biotome." The propriety of this arrangement will, I think, appear in the sequel. Let us, therefore, in the next place, glance at the life-phases of one of the cestodes. Those of *Tænia serrata* may be tabulated thus:—

ZOOLOGICAL INDIVIDUAL (*Tænia serrata*).

a. Ovum in all stages.	Primary "biotome."
b. Six-hooked embryo, boring larva, or proscolex.	
c. Resting larva, scolex, or *Cysticercus pisiformis*.	
d. Sexually immature tapeworm in all stages.	Secondary "biotome."
e. Mature tapeworm-colony, strobile, or *Tænia*.	
f. Segment, free-joint, or *proglottis* (zoöid). .	

According to Professor Huxley's views, in the above "individual" the stages *a*, *b*, *c*, *d*, would collectively represent the first life-phase or "protozoöid," whilst the final phase, *f*, would be the "deutero-zoöid." I have, indeed, with Professor Huxley's approval, so represented them in another part of this treatise (p. 222); but, recently, I have not been able to satisfy myself that the nomenclature in question meets all the requirements of the case. In my view, the six-hooked embryo is as much an individualised form as the *Cysticercus*-stage, whilst the latter is as much a life-phase as the *proglottis* itself. Why, therefore, may we not here recognize three zoöids (proto-deutero-trito-zoöids), instead of two only, after the manner suggested by Professor Huxley? If this view be accepted, our *Tænia serrata*, in its full zoological individuality, would be represented by two biotomes, the primary one comprising two individualised phases (the proscolex and scolex, or protozoöid and deuterozoöid), and the secondary one comprising a practically indefinite number of individualised forms or tritozoöids. In the

one case the independent life-phases are the result of metamorphosis, but in the other they are the product of gemmation. Let us next see how the matter stands in regard to one of the Trematodes, say, for example, the common liver-fluke (*Fasciola hepatica*), which may be tabulated as follows:—

ZOOLOGICAL INDIVIDUAL (*Fasciola hepatica*).

a. Ovum in all stages.	First "biotome."
b. Ciliated free-swimming embryo. . . .	
c Non-ciliated larva (nurse, germ-sac, sporocyst, *redia*).	Second "biotome."
d. Active, migrating, tailed larva (*cercaria*). .	
e Encysted, resting larva (*pupa*). . . .	Third "biotome."
f. Sexually mature fluke (*fasciola*). . .	

This is probably a fair representation of the ordinary fluke individual, in which species, however, the entire life-phases have not as yet been thoroughly identified. It is quite certain that the life-phases are never less numerous or complicated than is here indicated; whilst Pagenstecher's researches tend to prove that, under certain climatal conditions, the number of larval forms may vary considerably. In other words, the fluke individual does not comprise any definite number of "zoöids," although the kinds of zoöids are limited. In the present case I recognize three "biotomes." The first includes only one temporary, independent life-phase; this is the ciliated animalcule, which, in my view, possesses sufficient individualised life to entitle it to be recognized as a "protozoöid." The second "biotome" may in some cases comprise only a solitary, simple sporocyst or germ-sac (deuterozoöid); but an almost indefinite multiplication of new and independent germ-sacs, as well as other more highly organized "nurse-formations," may also be developed from the primary sporocyst (secondary and tertiary "deuterozoöids"). This reminds us of the practically indefinite number of zoöids (proglottides) which the second "biotome" of the Cestode gives rise to; but here there is analogy, and not homology. The third "biotome" embraces a large but variable number of "tritozoöids"

(cercariæ), an equal number (whatever that may be) of "tetartozoöids" *(pupæ)*, and, therefore also, a similar number of "pemptozoöids" (flukes).

Whether the views here promulgated be accepted or not, I have, I trust, made it sufficiently clear that the fluke-individual may comprise, in its life-cycle, a great and varying number of life-phases, each of which may or may not possess equivalent (and, necessarily, very limited) zoological value. The variability of the character of these life-phases is shown by the sporocysts (deuterozoöids), which are not only unequal to one another in bulk, but also in organization, the higher forms (rediæ) developing a rudimentary digestive apparatus. Apparently the *redia* is not, in all cases, an essential feature of Trematode larval life. Putting together the whole possible and independent life-phases, and placing their numerical development within the lowest limits, our ordinary fluke-individual would, I reckon, comprise about 370 "zoöid" formations, those of the second "biotome" being produced by the well-known process of internal gemmation, whilst those of the third "biotome" are the result of a simple yet prolonged metamorphosis.

I conceive that Dr. Pagenstecher's apparently well-established proposition (that "only such Trematode larvæ as are capable of arriving at sexual maturity are furnished with special appendages") gives strength to my views regarding the recognisable epochs in the fluke-individual's life, and points to the line of origin, continuity, and definiteness of the third "biotome" which I have recognized on totally independent grounds.* There is about the same relative amount of individualised-being in the caterpillar, pupa, and imago states of the Insect, as there is in the cercaria, pupa, and fluke conditions of the Trematode; but the "epoch" of the one embraces the whole life of the "zoological individual," whilst in the other it represents only a section, or "biotome," of the life-cycle. If the term "zoöid" be not

* See Pagenstecher's "Schlussbemerkungen" (*loc. cit.*, p. 29 of the present work).

allowable for the separate metamorphosed life-phases, as well as for true gemmæ, some other distinctive nomenclature must be substituted. I would like to see it retained to designate the semi-individualised, separable life-phase, without regard to its peculiar mode of genesis.

Practically, other curious results arise out of the foregoing considerations. For example, a single sheep may harbour 1000 flukes. Each fluke may develope 10,000 eggs. Each egg may give rise to 370 zoöids. It thus appears that, if all the conditions were favourable, a single fluke might originate between three and four millions of individualised life-forms; whilst the solitary sheep itself would, under the same circumstances, be the means of causing the production of at least 3,000,000,000 fluke-zoöids! Happily, no such result as this can possibly occur in nature, since a multitude of "interfering agencies" reduces the "favourable conditions" to a comparatively small number. However, the balance of parasitic forms from all sources is sufficient to destroy thousands of sheep annually—to say nothing of the wounds inflicted on millions of small mollusks, into whose bodies the "zoöids" penetrate.

Reverting to the Cestodes, the results attainable from particular species are, in some respects, still more striking. Let us separately examine the "zoological individuals" of *Tænia cœnurus* and *Tænia echinococcus*. The life-phases of the former may be tabulated as follows:—

Zoological individual (*Tænia cœnurus*).	
a. Ovum in all stages.	First "biotome."
b. Six-hooked embryo, boring larva, or *proscolex*. .	
c. Resting, polycephalous larva (*Cœnurus cerebralis*).	
d. Sexually immature tapeworm.	Second "biotome."
e. Mature tapeworm colony, or strobile. . . .	
f. Segment, free-joint, or *proglottis*. . . .	

At first sight, this representation appears to be the same as that of the *Tænia serrata*, already given. It is, in truth, zoologically equivalent, but the component life-phases are both struc-

turally and numerically different. The "resting larva" of *Tænia serrata* consists of a single free scolex, whilst the "resting larva" of *T. cœnurus* comprises a multitude of conjoined, inseparable *scolices*. In the one case the scolex is a true "zoöid," in the other it is the merest fraction of a "zoöid." In the case of *Tænia serrata*, one single egg, under the most favourable conditions, can only lead to the development of one tapeworm; but, under like circumstances, the single egg of *Tænia cœnurus* may lead to the formation of at least 300 tapeworms. This is accomplished when the *Cœnurus* of the sheep's brain is transferred to the stomach of the dog, and all the scolex-heads with which it is furnished become developed into tapeworms. If we call to our aid an estimate of the "zoöids," the result is very much more significant. On the plan of interpretation previously adopted, the "zoological individual" of *Tænia serrata* (allowing 500 proglottides for the strobile) would only yield us 503 "zooids" (as I have defined them); but in the case of *Tænia cœnurus* this representation would certainly give us as many as 1,500,000 "zoöids." Then, as regards the total number of eggs produced by all the final "zoöids" collectively, we should, in the case of *Tænia serrata* (allowing each proglottis to contain 5000 ova), obtain the comparatively small total of 2,500,000 eggs; whilst in the case of *Tænia cœnurus*, the progeny of a single germ would collectively give out no less than 7,500,000,000 ova!

Lastly, let us glance at the possible results derivable from a consideration of the "zoological individual" of *Tænia echinococcus*.

ZOOLOGICAL INDIVIDUAL (*Tænia echinococcus*).

a. Ovum in all stages.	First "biotome."
b. Six-hooked embryo, boring larva, or proscolex. .	
c. Resting, acephalocystic larva (hydatid). . .	
d. Sexually immature tapeworm.	Second "biotome."
e. Mature tapeworm colony, or strobile. . . .	
f. Segment, *proglottis*, or free-joint. . . .	

Here, again, the representation is as simple as obtained either

in the case of *Tænia serrata* or in that of *Tænia cœnurus*; but, in point of numerical and structural detail, the life-phases are remarkably different. Here, the "resting larva," as in *Cœnurus*, is furnished with a multitude of "heads;" these, the latter, being the well known echinococci or scolexes developed in a rather more complete form than occur in *Cœnurus cerebralis*. In fact, the so-called "heads" are almost separable "phases," being attached to the maternal larva by slender pedicles only. They are, indeed, frequently found detached; but then it is questionable if they have not already parted with their vitality. In this view I cannot call them "zoöids"; but the daughter-hydatid formations, which are developed within or without the original maternal hydatid, are quite deserving of such distinction. The latter are separable, organized life-phases, each of which, like its parent, may develope a multitude of echinococci; so that, under favourable conditions, there is practically no limit to the number of "heads" which may be generated by a proliferating hydatid; consequently, also, there is practically no limit to the number of tapeworms liable to be developed from the same source. The strobiles in this case, however, have only three joints capable of arriving at sexual maturity; at least, only the lowermost of these is mature at one and the same time. Whether or no the Tæniæ are susceptible of indefinite proglottis-multiplication, after the fashion of ordinary tapeworms, is a point on which I am, at present, uninformed; it is probable, however, that the joints follow the ordinary law of successional development. In either case our computation of the number of zoöids and eggs capable of arising from a single germ need not be affected by this consideration. Taking an average case of hydatid development, and assuming the existence of conditions favourable to the complete development of the entire progeny, a single germ of *Tænia echinococcus* might, without any exaggeration, give us between five and six million separate life-phases, or "zoöids," from which, under like circumstances, there would result not less than 150,000,000,000 ova! In this calcula-

tion I do not take into account the probability of each hydatid tapeworm or strobile developing more than three successive sexually mature segments, and I allow for each proglottis (tetartozoöid) only 10,000 eggs. For each hydatid I allow 10,000 scolices; though, doubtless, one large acephalocyst might develope ten times that number. As many as a thousand hydatids, may be discovered in the body of a single "host"; but, according to my experiences, the echinococcus-heads are not usually present in more than a limited number of the numerous daughter vesicles. If the hydatids were less "cribbed, cabined, and confined" than is commonly the case, no doubt can be entertained that their power of proliferating and of producing the so-called "heads" or scolices would be correspondingly increased.

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———— Scolecida; the same for the year 1867 (p. 639). London, 1868.

ERRATA.

In addition to the few errors indicated at the first page of this work, I have to point out the following:—

At page 103, line 9, for "with aquiferous," read "with the aquiferous."

At page 153, line 10, for "naturally lead," read "naturally leads."

At page 270, line 2 and elsewhere, for "Naumyn," read "Naunyn."

At page 279, line 8, for "Baillinger," read "Baillarger."

At page 286, line 12, for "were are," read "we are."

At page 313, line 10, for "of Dyer," read "of Professor Robert Dyce."

At page 337, line 15, for "Valentine," read "Valentin;" and at line 19 of same page, for "Rupprecht Fiedler," read "Rupprecht and Fiedler."

At page 339, in the footnote, for "Dr. Lieving," read "Dr. Liveing."

At page 369, line 8, for "and its tail, without exception, the flattened," read "and the tail are applied to the flattened side of the egg."

At page 442, in the Bibliography, for "Dyer," read "Dyce."

And at page 472, line 3, for "Roy. Soc.," read "Ray Society."—T. S. C.

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THE END.

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